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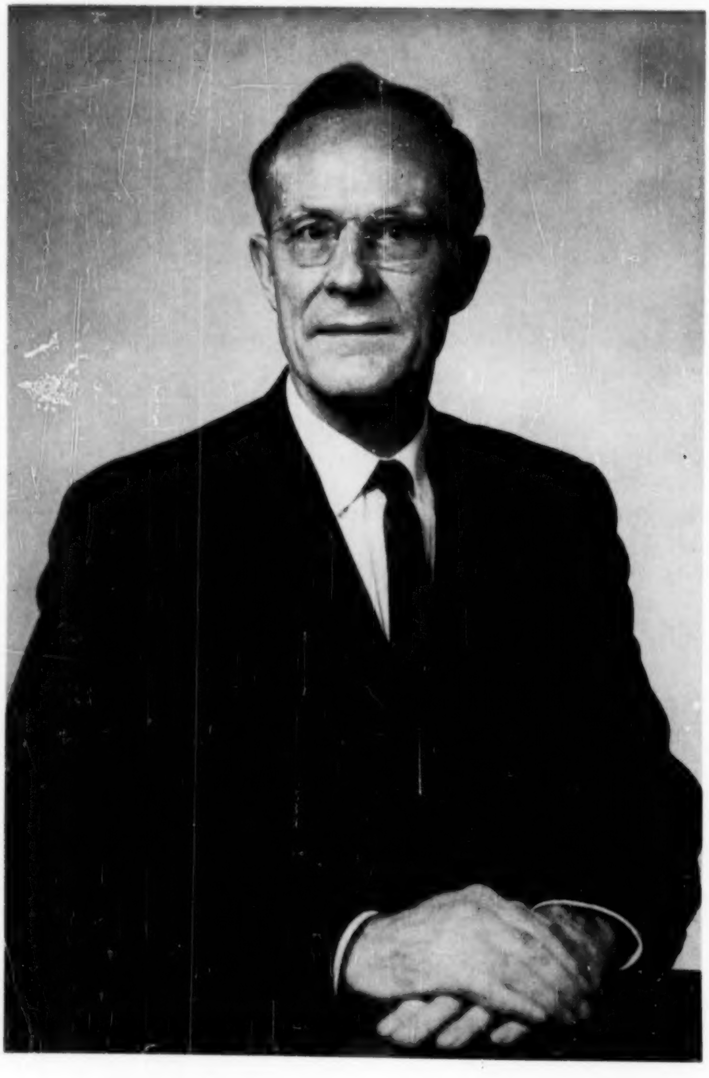

Theodore W. Schultz

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INVESTMENT IN HUMAN CAPITAL*

By Theodore W. Schultz

Although it is obvious that people acquire useful skills and knowledge, it is not obvious that these skills and knowledge are a form of capital, that this capital is in substantial part a product of deliberate investment, that it has grown in Western societies at a much faster rate than conventional (nonhuman) capital, and that its growth may well be the most distinctive feature of the economic system. It has been widely observed that increases in national output have been large compared with the increases of land, man-hours, and physical reproducible capital. Investment in human capital is probably the major explanation for this difference.

Much of what we call consumption constitutes investment in human capital. Direct expenditures on education, health, and internal migration to take advantage of better job opportunities are clear examples. Earnings foregone by mature students attending school and by workers acquiring on-the-job training are equally clear examples. Yet nowhere do these enter into our national accounts. The use of leisure time to improve skills and knowledge is widespread and it too is unrecorded. In these and similar ways the *quality* of human effort can be greatly improved and its productivity enhanced. I shall contend that such investment in human capital accounts for most of the impressive rise in the real earnings per worker.

I shall comment, first, on the reasons why economists have shied away from the explicit analysis of investment in human capital, and then, on the capacity of such investment to explain many a puzzle about economic growth. Mainly, however, I shall concentrate on the scope and substance of human capital and its formation. In closing I shall consider some social and policy implications.

* Presidential Address delivered at the Seventy-Third Annual Meeting of the American Economic Association, Saint Louis, December 28, 1960. The author is indebted to his colleagues Milton Friedman, for his very helpful suggestions to gain clarity and cogency, and Harry G. Johnson for pointing out a number of ambiguities.

I. *Shying Away from Investment in Man*

Economists have long known that people are an important part of the wealth of nations. Measured by what labor contributes to output, the productive capacity of human beings is now vastly larger than all other forms of wealth taken together. What economists have not stressed is the simple truth that people invest in themselves and that these investments are very large. Although economists are seldom timid in entering on abstract analysis and are often proud of being impractical, they have not been bold in coming to grips with this form of investment. Whenever they come even close, they proceed gingerly as if they were stepping into deep water. No doubt there are reasons for being wary. Deep-seated moral and philosophical issues are ever present. Free men are first and foremost the end to be served by economic endeavor; they are not property or marketable assets. And not least, it has been all too convenient in marginal productivity analysis to treat labor as if it were a unique bundle of innate abilities that are wholly free of capital.

The mere thought of investment in human beings is offensive to some among us.[1] Our values and beliefs inhibit us from looking upon human beings as capital goods, except in slavery, and this we abhor. We are not unaffected by the long struggle to rid society of indentured service and to evolve political and legal institutions to keep men free from bondage. These are achievements that we prize highly. Hence, to treat human beings as wealth that can be augmented by investment runs counter to deeply held values. It seems to reduce man once again to a mere material component, to something akin to property. And for man to look upon himself as a capital good, even if it did not impair his freedom, may seem to debase him. No less a person than J. S. Mill at one time insisted that the people of a country should not be looked upon as wealth because wealth existed only for the sake of people [15]. But surely Mill was wrong; there is nothing in the concept of human wealth contrary to his idea that it exists only for the advantage of people. By investing in themselves, people can enlarge the range of choice available to them. It is one way free men can enhance their welfare.

Among the few who have looked upon human beings as capital, there are three distinguished names. The philosopher-economist Adam Smith boldly included all of the acquired and useful abilities of all of the inhabitants of a country as a part of capital. So did H. von Thünen, who then went on to argue that the concept of capital applied to man did not degrade him or impair his freedom and dignity, but on the contrary that the failure to apply the concept was especially pernicious in wars; ". . . for here . . . one will sacrifice in a battle a hundred

[1] This paragraph draws on the introduction to my Teller Lecture [16].

human beings in the prime of their lives without a thought in order to save one gun." The reason is that, ". . . the purchase of a cannon causes an outlay of public funds, whereas human beings are to be had for nothing by means of a mere conscription decree" [20]. Irving Fisher also clearly and cogently presented an all-inclusive concept of capital [6]. Yet the main stream of thought has held that it is neither appropriate nor practical to apply the concept of capital to human beings. Marshall [11], whose great prestige goes far to explain why this view was accepted, held that while human beings are incontestably capital from an abstract and mathematical point of view, it would be out of touch with the market place to treat them as capital in practical analyses. Investment in human beings has accordingly seldom been incorporated in the formal core of economics, even though many economists, including Marshall, have seen its relevance at one point or another in what they have written.

The failure to treat human resources explicitly as a form of capital, as a produced means of production, as the product of investment, has fostered the retention of the classical notion of labor as a capacity to do manual work requiring little knowledge and skill, a capacity with which, according to this notion, laborers are endowed about equally. This notion of labor was wrong in the classical period and it is patently wrong now. Counting individuals who can and want to work and treating such a count as a measure of the quantity of an economic factor is no more meaningful than it would be to count the number of all manner of machines to determine their economic importance either as a stock of capital or as a flow of productive services.

Laborers have become capitalists not from a diffusion of the ownership of corporation stocks, as folklore would have it, but from the acquisition of knowledge and skill that have economic value [9]. This knowledge and skill are in great part the product of investment and, combined with other human investment, predominantly account for the productive superiority of the technically advanced countries. To omit them in studying economic growth is like trying to explain Soviet ideology without Marx.

II. *Economic Growth from Human Capital*

Many paradoxes and puzzles about our dynamic, growing economy can be resolved once human investment is taken into account. Let me begin by sketching some that are minor though not trivial.

When farm people take nonfarm jobs they earn substantially less than industrial workers of the same race, age, and sex. Similarly nonwhite urban males earn much less than white males even after allowance is made for the effects of differences in unemployment, age, city

size and region [21]. Because these differentials in earnings correspond closely to corresponding differentials in education, they strongly suggest that the one is a consequence of the other. Negroes who operate farms, whether as tenants or as owners, earn much less than whites on comparable farms.[2] Fortunately, crops and livestock are not vulnerable to the blight of discrimination. The large differences in earnings seem rather to reflect mainly the differences in health and education. Workers in the South on the average earn appreciably less than in the North or West and they also have on the average less education. Most migratory farm workers earn very little indeed by comparison with other workers. Many of them have virtually no schooling, are in poor health, are unskilled, and have little ability to do useful work. To urge that the differences in the amount of human investment may explain these differences in earnings seems elementary. Of more recent vintage are observations showing younger workers at a competitive advantage; for example, young men entering the labor force are said to have an advantage over unemployed older workers in obtaining satisfactory jobs. Most of these young people possess twelve years of school, most of the older workers six years or less. The observed advantage of these younger workers may therefore result not from inflexibilities in social security or in retirement programs, or from sociological preference of employers, but from real differences in productivity connected with one form of human investment, i.e., education. And yet another example, the curve relating income to age tends to be steeper for skilled than for unskilled persons. Investment in on-the-job training seems a likely explanation, as I shall note later.

Economic growth requires much internal migration of workers to adjust to changing job opportunities [10]. Young men and women move more readily than older workers. Surely this makes economic sense when one recognizes that the costs of such migration are a form of human investment. Young people have more years ahead of them than older workers during which they can realize on such an investment. Hence it takes less of a wage differential to make it economically advantageous for them to move, or, to put it differently, young people can expect a higher return on their investment in migration than older people. This differential may explain selective migration without requiring an appeal to sociological differences between young and old people.

The examples so far given are for investment in human beings that yield a return over a long period. This is true equally of investment in education, training, and migration of young people. Not all investments in human beings are of this kind; some are more nearly akin to current inputs as for example expenditures on food and shelter in some coun-

[2] Based on unpublished preliminary results obtained by Joseph Willett in his Ph.D. research at the University of Chicago.

tries where work is mainly the application of brute human force, calling for energy and stamina, and where the intake of food is far from enough to do a full day's work. On the "hungry" steppes and in the teeming valleys of Asia, millions of adult males have so meager a diet that they cannot do more than a few hours of hard work. To call them underemployed does not seem pertinent. Under such circumstances it is certainly meaningful to treat food partly as consumption and partly as a current "producer good," as some Indian economists have done [3]. Let us not forget that Western economists during the early decades of industrialization and even in the time of Marshall and Pigou often connected additional food for workers with increases in labor productivity.

Let me now pass on to three major perplexing questions closely connected with the riddle of economic growth. First, consider the long-period behavior of the capital-income ratio. We were taught that a country which amassed more reproducible capital relative to its land and labor would employ such capital in greater "depth" because of its growing abundance and cheapness. But apparently this is not what happens. On the contrary, the estimates now available show that less of such capital tends to be employed relative to income as economic growth proceeds. Are we to infer that the ratio of capital to income has no relevance in explaining either poverty or opulence? Or that a rise of this ratio is not a prerequisite to economic growth? These questions raise fundamental issues bearing on motives and preferences for holding wealth as well as on the motives for particular investments and the stock of capital thereby accumulated. For my purpose all that needs to be said is that these estimates of capital-income ratios refer to only a part of all capital. They exclude in particular, and most unfortunately, any human capital. Yet human capital has surely been increasing at a rate substantially greater than reproducible (nonhuman) capital. We cannot, therefore, infer from these estimates that the stock of *all* capital has been decreasing relative to income. On the contrary, if we accept the not implausible assumption that the motives and preferences of people, the technical opportunities open to them, and the uncertainty associated with economic growth during particular periods were leading people to maintain roughly a constant ratio between *all* capital and income, the decline in the estimated capital-income ratio[3] is simply a signal that human capital has been increasing relatively not only to conventional capital but also to income.

The bumper crop of estimates that show national income increas-

[3] I leave aside here the difficulties inherent in identifying and measuring both the nonhuman capital and the income entering into estimates of this ratio. There are index number and aggregation problems aplenty, and not all improvements in the quality of this capital have been accounted for, as I shall note later.

ing faster than national resources raises a second and not unrelated puzzle. The income of the United States has been increasing at a much higher rate than the combined amount of land, man-hours worked and the stock of reproducible capital used to produce the income. Moreover, the discrepancy between the two rates has become larger from one business cycle to the next during recent decades [5]. To call this discrepancy a measure of "resource productivity" gives a name to our ignorance but does not dispel it. If we accept these estimates, the connections between national resources and national income have become loose and tenuous over time. Unless this discrepancy can be resolved, received theory of production applied to inputs and outputs as currently measured is a toy and not a tool for studying economic growth.

Two sets of forces probably account for the discrepancy, if we neglect entirely the index number and aggregation problems that bedevil all estimates of such global aggregates as total output and total input. One is returns to scale; the second, the large improvements in the quality of inputs that have occurred but have been omitted from the input estimates. Our economy has undoubtedly been experiencing increasing returns to scale at some points offset by decreasing returns at others. If we can succeed in identifying and measuring the net gains, they may turn out to have been substantial. The improvements in the quality of inputs that have not been adequately allowed for are no doubt partly in material (nonhuman) capital. My own conception, however, is that both this defect and the omission of economies of scale are minor sources of discrepancy between the rates of growth of inputs and outputs compared to the improvements in human capacity that have been omitted.

A small step takes us from these two puzzles raised by existing estimates to a third which brings us to the heart of the matter, namely the essentially unexplained large increase in real earnings of workers. Can this be a windfall? Or a quasirent pending the adjustment in the supply of labor? Or, a pure rent reflecting the fixed amount of labor? It seems far more reasonable that it represents rather a return to the investment that has been made in human beings. The observed growth in productivity per unit of labor is simply a consequence of holding the unit of labor constant over time although in fact this unit of labor has been increasing as a result of a steadily growing amount of human capital per worker. As I read our record, the human capital component has become very large as a consequence of human investment.

Another aspect of the same basic question, which admits of the same resolution, is the rapid postwar recovery of countries that had suffered severe destruction of plant and equipment during the war. The toll from bombing was all too visible in the factories laid flat, the railroad

yards, bridges, and harbors wrecked, and the cities in ruin. Structures, equipment and inventories were all heaps of rubble. Not so visible, yet large, was the toll from the wartime depletion of the physical plant that escaped destruction by bombs. Economists were called upon to assess the implications of these wartime losses for recovery. In retrospect, it is clear that they overestimated the prospective retarding effects of these losses. Having had a small hand in this effort, I have had a special reason for looking back and wondering why the judgments that we formed soon after the war proved to be so far from the mark. The explanation that now is clear is that we gave altogether too much weight to nonhuman capital in making these assessments. We fell into this error, I am convinced, because we did not have a concept of *all* capital and, therefore, failed to take account of human capital and the important part that it plays in production in a modern economy.

Let me close this section with a comment on poor countries, for which there are virtually no solid estimates. I have been impressed by repeatedly expressed judgments, especially by those who have a responsibility in making capital available to poor countries, about the low rate at which these countries can absorb additional capital. New capital from outside can be put to good use, it is said, only when it is added "slowly and gradually." But this experience is at variance with the widely held impression that countries are poor fundamentally because they are starved for capital and that additional capital is truly the key to their more rapid economic growth. The reconciliation is again, I believe, to be found in emphasis on particular forms of capital. The new capital available to these countries from outside as a rule goes into the formation of structures, equipment and sometimes also into inventories. But it is generally not available for additional investment in man. Consequently, human capabilities do not stay abreast of physical capital, and they do become limiting factors in economic growth. It should come as no surprise, therefore, that the absorption rate of capital to augment only particular nonhuman resources is necessarily low. The Horvat [8] formulation of the optimum rate of investment which treats knowledge and skill as a critical investment variable in determining the rate of economic growth is both relevant and important.

III. *Scope and Substance of These Investments*

What are human investments? Can they be distinguished from consumption? Is it at all feasible to identify and measure them? What do they contribute to income? Granted that they seem amorphous compared to brick and mortar, and hard to get at compared to the investment accounts of corporations, they assuredly are not a fragment;

they are rather like the contents of Pandora's box, full of difficulties and hope.

Human resources obviously have both quantitative and qualitative dimensions. The number of people, the proportion who enter upon useful work, and hours worked are essentially quantitative characteristics. To make my task tolerably manageable, I shall neglect these and consider only such quality components as skill, knowledge, and similar attributes that affect particular human capabilities to do productive work. In so far as expenditures to enhance such capabilities also increase the value productivity of human effort (labor), they will yield a positive rate of return.[4]

How can we estimate the magnitude of human investment? The practice followed in connection with physical capital goods is to estimate the magnitude of capital formation by expenditures made to produce the capital goods. This practice would suffice also for the formation of human capital. However, for human capital there is an additional problem that is less pressing for physical capital goods: how to distinguish between expenditures for consumption and for investment. This distinction bristles with both conceptual and practical difficulties. We can think of three classes of expenditures: expenditures that satisfy consumer preferences and in no way enhance the capabilities under discussion—these represent pure consumption; expenditures that enhance capabilities and do not satisfy any preferences underlying consumption—these represent pure investment; and expenditures that have both effects. Most relevant activities clearly are in the third class, partly consumption and partly investment, which is why the task of identifying each component is so formidable and why the measurement of capital formation by expenditures is less useful for human investment than for investment in physical goods. In principle there is an alternative method for estimating human investment, namely by its yield rather than by its cost. While any capability produced by human investment becomes a part of the human agent and hence cannot be sold; it is nevertheless "in touch with the market place" by affecting the wages and salaries the human agent can earn. The resulting increase in earnings is the yield on the investment.[5]

Despite the difficulty of exact measurement at this stage of our understanding of human investment, many insights can be gained by examining some of the more important activities that improve human

[4] Even so, our *observed* return can be either negative, zero or positive because our observations are drawn from a world where there is uncertainty and imperfect knowledge and where there are windfall gains and losses and mistakes aplenty.

[5] In principle, the value of the investment can be determined by discounting the additional future earnings it yields just as the value of a physical capital good can be determined by discounting its income stream.

capabilities. I shall concentrate on five major categories: (1) health facilities and services, broadly conceived to include all expenditures that affect the life expectancy, strength and stamina, and the vigor and vitality of a people; (2) on-the-job training, including old-style apprenticeship organized by firms; (3) formally organized education at the elementary, secondary, and higher levels; (4) study programs for adults that are not organized by firms, including extension programs notably in agriculture; (5) migration of individuals and families to adjust to changing job opportunities. Except for education, not much is known about these activities that is germane here. I shall refrain from commenting on study programs for adults, although in agriculture the extension services of the several states play an important role in transmitting new knowledge and in developing skills of farmers [17]. Nor shall I elaborate further on internal migration related to economic growth.

Health activities have both quantity and quality implications. Such speculations as economists have engaged in about the effects of improvements in health,[6] has been predominantly in connection with population growth, which is to say with quantity. But surely health measures also enhance the quality of human resources. So also may additional food and better shelter, especially in underdeveloped countries.

The change in the role of food as people become richer sheds light on one of the conceptual problems already referred to. I have pointed out that extra food in some poor countries has the attribute of a "producer good." This attribute of food, however, diminishes as the consumption of food rises, and there comes a point at which any further increase in food becomes pure consumption.[7] Clothing, housing and perhaps medical services may be similar.

My comment about on-the-job training will consist of a conjecture on the amount of such training, a note on the decline of apprenticeship, and then a useful economic theorem on who bears the costs of such training. Surprisingly little is known about on-the-job training in modern industry. About all that can be said is that the expansion of education has not eliminated it. It seems likely, however, that some of the training formerly undertaken by firms has been discontinued and other training programs have been instituted to adjust both to the

[6] Health economics is in its infancy; there are two medical journals with "economics" in their titles, two bureaus for economic research in private associations (one in the American Medical and the other in the American Dental Association), and not a few studies and papers by outside scholars. Selma Mushkin's survey is very useful with its pertinent economic insights, though she may have underestimated somewhat the influence of the economic behavior of people in striving for health [14].

[7] For instance, the income elasticity of the demand for food continues to be positive even after the point is reached where additional food no longer has the attribute of a "producer good."

rise in the education of workers and to changes in the demands for new skills. The amount invested annually in such training can only be a guess. H. F. Clark places it near to equal to the amount spent on formal education.[8] Even if it were only one-half as large, it would represent currently an annual gross investment of about $15 billion. Elsewhere, too, it is thought to be important. For example, some observers have been impressed by the amount of such training under way in plants in the Soviet Union.[9] Meanwhile, apprenticeship has all but disappeared, partly because it is now inefficient and partly because schools now perform many of its functions. Its disappearance has been hastened no doubt by the difficulty of enforcing apprenticeship agreements. Legally they have come to smack of indentured service. The underlying economic factors and behavior are clear enough. The apprentice is prepared to serve during the initial period when his productivity is less than the cost of his keep and of his training. Later, however, unless he is legally restrained, he will seek other employment when his productivity begins to exceed the cost of keep and training, which is the period during which a master would expect to recoup on his earlier outlay.

To study on-the-job training Gary Becker [1] advances the theorem that in competitive markets employees pay all the costs of their training and none of these costs are ultimately borne by the firm. Becker points out several implications. The notion that expenditures on training by a firm generate external economies for other firms is not consistent with this theorem. The theorem also indicates one force favoring the transfer from on-the-job training to attending school. Since on-the-job training reduces the net earnings of workers at the beginning and raises them later on, this theorem also provides an explanation for the "steeper slope of the curve relating income to age," for skilled than unskilled workers, referred to earlier.[10] What all this adds up to is that the stage is set to undertake meaningful economic studies of on-the-job training.

Happily we reach firmer ground in regard to education. Investment in education has risen at a rapid rate and by itself may well account for a substantial part of the otherwise unexplained rise in earnings. I shall do no more than summarize some preliminary results about the total costs of education including income foregone by students, the apparent relation of these costs to consumer income and to alternative invest-

[8] Based on comments made by Harold F. Clark at the Merrill Center for Economics, summer 1959; also, see [4].

[9] Based on observations made by a team of U. S. economists of which I was a member, see *Saturday Rev.*, Jan. 21, 1961.

[10] Becker has also noted still another implication arising out of the fact that the income and capital investment aspects of on-the-job training are tied together, which gives rise to "permanent" and "transitory" income effects that may have substantial explanatory value.

ments, the rise of the stock of education in the labor force, returns to education, and the contribution that the increase in the stock of education may have made to earnings and to national income.

It is not difficult to estimate the conventional costs of education consisting of the costs of the services of teachers, librarians, administrators, of maintaining and operating the educational plant, and interest on the capital embodied in the educational plant. It is far more difficult to estimate another component of total cost, the income foregone by students. Yet this component should be included and it is far from negligible. In the United States, for example, well over half of the costs of higher education consists of income foregone by students. As early as 1900, this income foregone accounted for about one-fourth of the total costs of elementary, secondary and higher education. By 1956, it represented over two-fifths of all costs. The rising significance of foregone income has been a major factor in the marked upward trend in the total real costs of education which, measured in current prices, increased from $400 million in 1900 to $28.7 billion in 1956 [18]. The percentage rise in educational costs was about three and a half times as large as in consumer income, which would imply a high income elasticity of the demand for education, if education were regarded as pure consumption.[11] Educational costs also rose about three and a half times as rapidly as did the gross formation of physical capital in dollars. If we were to treat education as pure investment this result would suggest that the returns to education were relatively more attractive than those to nonhuman capital.[12]

Much schooling is acquired by persons who are not treated as income earners in most economic analysis, particularly, of course, women. To analyze the effect of growth in schooling on earnings, it is therefore necessary to distinguish between the stock of education in the population and the amount in the labor force. Years of school completed are far from satisfactory as a measure because of the marked increases that have taken place in the number of days of school attendance of enrolled students and because much more of the education of workers consists of high school and higher education than formerly. My preliminary estimates suggest that the stock of education in the labor force rose about eight and a half times between 1900 and 1956, whereas the stock of reproducible capital rose four and a half times, both in 1956 prices. These estimates are, of course, subject to many

[11] Had other things stayed constant this suggests an income elasticity of 3.5. Among the things that did change, the prices of educational services rose relative to other consumer prices, perhaps offset in part by improvements in the quality of educational services.

[12] This of course assumes among other things that the relationship between gross and net have not changed or have changed in the same proportion. Estimates are from my essay, "Education and Economic Growth" [19].

qualifications.[13] Nevertheless, both the magnitude and the rate of increase of this form of human capital have been such that they could be an important key to the riddle of economic growth.[14]

The exciting work under way is on the return to education. In spite of the flood of high school and college graduates, the return has not become trivial. Even the lower limits of the estimates show that the return to such education has been in the neighborhood of the return to nonhuman capital. This is what most of these estimates show when they treat as costs all of the public and private expenditures on education and also the income foregone while attending school, and when they treat all of these costs as investment, allocating none to consumption.[15] But surely a part of these costs are consumption in the sense that education creates a form of consumer capital[16] which has the attribute of

[13] From [19, Sec. 4]. These estimates of the stock of education are tentative and incomplete. They are incomplete in that they do not take into account fully the increases in the average life of this form of human capital arising out of the fact that relatively more of this education is held by younger people in the labor force than was true in earlier years; and, they are incomplete because no adjustment has been made for the improvements in education over time, increasing the quality of a year of school in ways other than those related to changes in the proportions represented by elementary, high school and higher education. Even so the stock of this form of human capital rose 8.5 times between 1900 and 1956 while the stock of reproducible nonhuman capital increased only 4.5 times, both in constant 1956 prices.

[14] In value terms this stock of education was only 22 per cent as large as the stock of reproducible physical capital in 1900, whereas in 1956 it already had become 42 per cent as large.

[15] Several comments are called for here. (1) The return to high school education appears to have declined substantially between the late 'thirties and early 'fifties and since then has leveled off, perhaps even risen somewhat, indicating a rate of return toward the end of the 'fifties about as high as that to higher education. (2) The return to college education seems to have risen somewhat since the late 'thirties in spite of the rapid influx of college-trained individuals into the labor force. (3) Becker's estimates based on the difference in income between high school and college graduates based on urban males adjusted for ability, race, unemployment and mortality show a return of 9 per cent to total college costs including both earnings foregone and conventional college costs, public and private and with none of these costs allocated to consumption (see his paper given at the American Economic Association meeting, December 1959 [2]). (4) The returns to this education in the case of nonwhite urban males, of rural males, and of females in the labor force may have been somewhat lower (see Becker [2]). (5) My own estimates, admittedly less complete than those of Becker and thus subject to additional qualifications, based mainly on lifetime income estimates of Herman P. Miller [12], lead to a return of about 11 per cent to both high school and college education as of 1958. See [19, Sec. 5].

Whether the consumption component in education will ultimately dominate, in the sense that the investment component in education will diminish as these expenditures increase and a point will be reached where additional expenditures for education will be pure consumption (a zero return on however small a part one might treat as an investment), is an interesting speculation. This may come to pass, as it has in the case of food and shelter, but that eventuality appears very remote presently in view of the prevailing investment value of education and the new demands for knowledge and skill inherent in the nature of our technical and economic progress.

[16] The returns on this consumer capital will not appear in the wages and salaries that people earn.

improving the taste and the quality of consumption of students throughout the rest of their lives. If one were to allocate a substantial fraction of the total costs of this education to consumption, say one-half, this would, of course, double the observed rate of return to what would then become the investment component in education that enhances the productivity of man.

Fortunately, the problem of allocating the costs of education in the labor force between consumption and investment does not arise to plague us when we turn to the contribution that education makes to earnings and to national income because a change in allocation only alters the rate of return, not the total return. I noted at the outset that the unexplained increases in U. S. national income have been especially large in recent decades. On one set of assumptions, the unexplained part amounts to nearly three-fifths of the total increase between 1929 and 1956.[17] How much of this unexplained increase in income represents a return to education in the labor force? A lower limit suggests that about three-tenths of it, and an upper limit does not rule out that more than one-half of it came from this source.[18] These estimates also imply that between 36 and 70 per cent of the hitherto unexplained rise in the earnings of labor is explained by returns to the additional education of workers.

IV. *A Concluding Note on Policy*

One proceeds at his own peril in discussing social implications and policy. The conventional hedge is to camouflage one's values and to wear the mantle of academic innocence. Let me proceed unprotected!

1. Our tax laws everywhere discriminate against human capital. Although the stock of such capital has become large and even though it is obvious that human capital, like other forms of reproducible capital, depreciates, becomes obsolete, and entails maintenance, our tax laws are all but blind on these matters.

2. Human capital deteriorates when it is idle because unemployment impairs the skills that workers have acquired. Losses in earnings can be cushioned by appropriate payments but these do not keep idleness from taking its toll from human capital.

3. There are many hindrances to the free choice of professions.

[17] Real income doubled, rising from $150 to $302 billion in 1956 prices. Eighty-nine billions of the increase in real income is taken to be unexplained, or about 59 per cent of the total increase. The stock of education in the labor force rose by $355 billion of which $69 billion is here allocated to the growth in the labor force to keep the per-worker stock of education constant, and $286 billion represents the increase in the level of this stock. See [19, Sec. 6] for an elaboration of the method and the relevant estimates.

[18] In per cent, the lower estimate came out to 29 per cent and the upper estimate to 56 per cent.

Racial discrimination and religious discrimination are still widespread. Professional associations and governmental bodies also hinder entry; for example, into medicine. Such purposeful interference keeps the investment in this form of human capital substantially below its optimum [7].

4. It is indeed elementary to stress the greater imperfections of the capital market in providing funds for investment in human beings than for investment in physical goods. Much could be done to reduce these imperfections by reforms in tax and banking laws and by changes in banking practices. Long-term private and public loans to students are warranted.

5. Internal migration, notably the movement of farm people into industry, made necessary by the dynamics of our economic progress, requires substantial investments. In general, families in which the husbands and wives are already in the late thirties cannot afford to make these investments because the remaining payoff period for them is too short. Yet society would gain if more of them would pull stakes and move because, in addition to the increase in productivity currently, the children of these families would be better located for employment when they were ready to enter the labor market. The case for making some of these investments on public account is by no means weak. Our farm programs have failed miserably these many years in not coming to grips with the costs and returns from off-farm migration.

6. The low earnings of particular people have long been a matter of public concern. Policy all too frequently concentrates only on the effects, ignoring the causes. No small part of the low earnings of many Negroes, Puerto Ricans, Mexican nationals, indigenous migratory farm workers, poor farm people and some of our older workers, reflects the failure to have invested in their health and education. Past mistakes are, of course, bygones, but for the sake of the next generation we can ill afford to continue making the same mistakes over again.

7. Is there a substantial underinvestment in human beings other than in these depressed groups? [2] This is an important question for economists. The evidence at hand is fragmentary. Nor will the answer be easily won. There undoubtedly have been overinvestments in some skills, for example, too many locomotive firemen and engineers, too many people trained to be farmers, and too many agricultural economists! Our schools are not free of loafers and some students lack the necessary talents. Nevertheless, underinvestment in knowledge and skill, relative to the amounts invested in nonhuman capital would appear to be the rule and not the exception for a number of reasons. The strong and increasing demands for this knowledge and skill in laborers are of fairly recent origin and it takes time to respond to them. In re-

sponding to these demands, we are heavily dependent upon cultural and political processes, and these are slow and the lags are long compared to the behavior of markets serving the formation of nonhuman capital. Where the capital market does serve human investments, it is subject to more imperfections than in financing physical capital. I have already stressed the fact that our tax laws discriminate in favor of nonhuman capital. Then, too, many individuals face serious uncertainty in assessing their innate talents when it comes to investing in themselves, especially through higher education. Nor is it easy either for public decisions or private behavior to untangle and properly assess the consumption and the investment components. The fact that the return to high school and to higher education has been about as large as the return to conventional forms of capital when all of the costs of such education including income foregone by students are allocated to the investment component, creates a strong presumption that there has been underinvestment since, surely, much education is cultural and in that sense it is consumption. It is no wonder, in view of these circumstances, that there should be substantial underinvestment in human beings, even though we take pride, and properly so, in the support that we have given to education and to other activities that contribute to such investments.

8. Should the returns from public investment in human capital accrue to the individuals in whom it is made?[19] The policy issues implicit in this question run deep and they are full of perplexities pertaining both to resource allocation and to welfare. Physical capital that is formed by public investment is not transferred as a rule to particular individuals as a gift. It would greatly simplify the allocative process if public investment in human capital were placed on the same footing. What then is the logical basis for treating public investment in human capital differently? Presumably it turns on ideas about welfare. A strong welfare goal of our community is to reduce the unequal distribution of personal income among individuals and families. Our community has relied heavily on progressive income and inheritance taxation. Given public revenue from these sources, it may well be true that public investment in human capital, notably that entering into general education, is an effective and efficient set of expenditures for attaining this goal. Let me stress, however, that the state of knowledge about these issues is woefully meager.

9. My last policy comment is on assistance to underdeveloped countries to help them achieve economic growth. Here, even more than in domestic affairs, investment in human beings is likely to be underrated

[19] I am indebted to Milton Friedman for bringing this issue to the fore in his comments on an early draft of this paper. See preface of [7] and also Jacob Mincer's pioneering paper [13].

and neglected. It is inherent in the intellectual climate in which leaders and spokesmen of many of these countries find themselves. Our export of growth doctrines has contributed. These typically assign the stellar role to the formation of nonhuman capital, and take as an obvious fact the superabundance of human resources. Steel mills are the real symbol of industrialization. After all, the early industrialization of England did not depend on investments in the labor force. New funds and agencies are being authorized to transfer capital for physical goods to these countries. The World Bank and our Export-Import Bank have already had much experience. Then, too, measures have been taken to pave the way for the investment of more private (nonhuman) capital abroad. This one-sided effort is under way in spite of the fact that the knowledge and skills required to take on and use efficiently the superior techniques of production, the most valuable resource that we could make available to them, is in very short supply in these underdeveloped countries. Some growth of course can be had from the increase in more conventional capital even though the labor that is available is lacking both in skill and knowledge. But the rate of growth will be seriously limited. It simply is not possible to have the fruits of a modern agriculture and the abundance of modern industry without making large investments in human beings.

Truly, the most distinctive feature of our economic system is the growth in human capital. Without it there would be only hard, manual work and poverty except for those who have income from property. There is an early morning scene in Faulkner's *Intruder in the Dust*, of a poor, solitary cultivator at work in a field. Let me paraphrase that line, "The man without skills and knowledge leaning terrifically against nothing."

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COMPARATIVE ADVANTAGE AND DEVELOPMENT POLICY

By Hollis B. Chenery*

In the great revival of interest in economic development that has marked the past decade, attention has centered on two main questions: first, what determines the over-all rate of economic advance?; second, what is the optimal allocation of given resources to promote growth? Analysis of the growth rate has relied mainly on the Keynesian tools and has produced a multiplicity of aggregate growth models. The second question, however, reopens more ancient economic issues, and their analysis must start from the classical and neoclassical solutions. Only very recently have the two types of discussion tended to come together in the more comprehensive framework of general equilibrium analysis.

In the field of resource allocation, controversy centers around the implications of the classical principle of comparative advantage, according to which growth is promoted by specialization. The defenders of this principle draw their inspiration from David Ricardo, J. S. Mill and Alfred Marshall, while the lines of attack stem from Friedrich List, J. A. Schumpeter, A. A. Young and J. H. Williams. The chief criticism is that comparative advantage is essentially a static concept which ignores a variety of dynamic elements.

This issue is of great practical importance to the governments of underdeveloped countries, most of which take an active part in allocating investment funds and other scarce resources. The main purpose of the discussion has therefore been to discover workable principles for the formulation of development policy. The classical approach derives these principles from international trade theory, while its critics base their analysis on modern growth theory. Elements of a dynamic, general-equilibrium theory are needed to resolve the differences between the two approaches. The more general analysis is of very limited value, however, unless its empirical implications can be ascertained.

The present paper discusses the analysis of resource allocation in

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less developed economies from three points of view. Section I tries to ascertain the extent to which the allocation principles derived from trade theory and from growth theory can be reconciled with each other without losing their operational significance. Section II compares various approaches to the measurement of optimal resource allocation in terms of their logical consistency and their applicability to different conditions. Section III examines some of the practical procedures followed in setting investment policy in underdeveloped countries in the light of the earlier discussion. Finally, some of the theoretical issues are re-examined to indicate their practical importance.

I. *Conflicts Between Trade Theory and Growth Theory*

The main contradictions between comparative advantage and other principles of resource allocation derive from their different orientation and assumptions. The classical analysis focuses on long-run tendencies and equilibrium conditions, while modern theories of growth are concerned with the interaction among producing and consuming units in a dynamic system. Since both approaches are familiar, I shall only try to identify the differences in assumptions and emphasis that lead to different policy conclusions.

A. *The Implications of Comparative Advantage for Resource Allocation*

The modern version of the comparative cost doctrine [20] is essentially a simplified form of static general equilibrium theory.[1] The optimum pattern of production and trade for a country is determined from a comparison of the opportunity cost of producing a given commodity with the price at which the commodity can be imported or exported. In equilibrium, no commodity is produced which could be imported at lower cost, and exports are expanded until marginal revenue equals marginal cost. Under the assumptions of full employment and perfect competition, the opportunity cost of a commodity, which is the value of the factors used to produce it in their best alternative employment, is equal to its market value. Market prices of factors and commodities can therefore be used to determine comparative advantage under competitive conditions. Long-term changes are not ignored, but they are assumed to be reflected in current market prices.

The Heckscher-Ohlin version of the comparative cost doctrine has been widely recommended as a basis for development policy because it provides a measure of comparative advantage that does not depend on the existence of perfect competition and initial equilibrium. This ver-

[1] An excellent discussion and synthesis of the several versions of trade theory is given by Caves [7]. The terms "comparative advantage" and "comparative cost" are used interchangeably in most discussions.

sion states that a country will benefit from trade by producing commodities that use more of its relatively abundant factors of production. It will export these commodities and import commodities using more of its relatively scarce factors unless its pattern of domestic demand happens to be biased toward commodities using domestic factors. The critical assumptions in this analysis are that factors of production are comparable among countries and that production functions are the same. These assumptions are not required by classical trade theory.

The applicability of the comparative cost doctrine to present-day conditions in underdeveloped countries has been re-examined by Viner and its validity has been reaffirmed with some modifications. Viner criticizes the Heckscher-Ohlin version because its assumption of comparable factors does not allow for observable differences in their quality [63, p. 16]. In his recent answer to critics of the comparative cost approach [64], however, Viner admits the necessity of interpreting comparative advantage in a dynamic setting in which the efficiency of production may change over time, external economies may exist, and the market prices of commodities and factors may differ from their opportunity cost. As Nurkse points out [64, p. 76], these modifications rob the original doctrine of much of its practical value. It is now necessary to have an explicit analysis of the growth process itself before it is possible to determine, even theoretically, where comparative advantage lies; market prices and current opportunity costs are no longer sufficient.

B. *Implications of Growth Theory for Resource Allocation*

Modern growth theory is concerned with the interactions over time among producers, consumers, and investors in interrelated sectors of the economy. In the writings of such economists as Rosenstein-Rodan [43], Lewis [29], Nurkse [36], Myrdal [34], Rostow [44], Dobb [12], and Hirschman [23], there is much more emphasis on the sequence of expansion of production and factor use by sector than on the conditions of general equilibrium. Growth theory either ignores comparative advantage and the possibilities of trade completely, or it considers mainly the dynamic aspects, such as the stimulus that an increase in exports provides to the development of related sectors or the function of imports as a carrier of new products and advanced technology. With this different point of view, growth theorists often suggest investment criteria that are quite contradictory to those derived from considerations of comparative advantage.

The conflicts between these two approaches to resource allocation may be traced either to differences in assumptions or to the inclusion of factors in one theory that are omitted from the other. Growth theory

contains at least four basic assumptions about underdeveloped economies that differ strongly from those underlying the comparative cost doctrine: (1) factor prices do not necessarily reflect opportunity costs with any accuracy; (2) the quantity and quality of factors of production may change substantially over time, in part as a result of the production process itself; (3) economies of scale relative to the size of existing markets are important in a number of sectors of production; (4) complementarity among commodities is dominant in both producer and consumer demand.

Some of the implications of these factors are developed by Rosenstein-Rodan [43] and Nurkse [36] as arguments for "balanced growth," by which is meant simultaneous expansion of a number of sectors of production.[2] Assuming an elastic supply of either capital or labor, these authors show that investment will be more profitable in related sectors, because of horizontal and vertical interdependence, than in the same sectors considered separately. Market forces will not necessarily lead to optimal investment decisions because present prices do not reflect the cost and demand conditions that will exist in the future. This effect of investment in one sector on the profitability of investment in another sector, via increased demand or reduced costs, has been called by Scitovsky [47] a "dynamic external economy." The imputation of these economies to the originating sectors may seriously affect the estimate of comparative advantage.

If we assume fixed investment resources instead of an elastic supply, the same set of factors provide an argument for concentrated or unbalanced growth [48] [50]. In order to achieve economies of scale in one sector, it may be necessary to devote a large fraction of the available investment funds to that sector and to supply increased requirements in other sectors from imports (or to curtail them temporarily). The optimal pattern of investment will then be one which concentrates first on one sector and then on another, with balance being approached only in the long run. Streeten [53] has developed further dynamic arguments for unbalanced growth from the fact that technological progress may be more rapid if increases in production are concentrated in a few sectors, while Hirschman [23] argues for imbalance to economize on entreprenurial ability.

The historical significance of the balanced growth argument has been examined by Gerschenkron [18], Rostow [44], and Ohlin [38], in the context of nineteenth-century industrial development in Europe. They show that vertical interdependence has been important in stimulating the growth of related industrial sectors, although the nature and

[2] The term "balanced growth" has been given a variety of meanings, but the idea of simultaneous expansion on several fronts is common to all of them.

origin of these complexes differ from country to country. In one case they may be related to exports, in another to expansion for the domestic market. The importance of interdependence among producers emerges fairly clearly from these historical studies.

The net effect of the discussion of dynamic interdependence and balanced vs. unbalanced growth is to destroy the presumption that perfect competition, even if it could be achieved, would lead to the optimum allocation of resources over time. Since the doctrine of comparative advantage in its conventional form is a corollary of general equilibrium theory, the theoretical qualifications that apply to the latter also apply to the former. If, then, the doctrine of comparative advantage is to be useful for development policy, the essential elements of the growth analysis must be combined with it.

C. *Dynamic Modifications of Comparative Advantage*

Classical trade theory does not exclude changes in the supply of factors and other data over time, but it does insist that under perfect competition the effects of such changes will be reflected in the market mechanism. If, on the other hand, we take comparative advantage as a principle of planning rather than as a result of market forces, we can include any foreseeable exogenous changes in technology, tastes, or other data without going beyond the framework of comparative statics.

Some of the modifications suggested by growth theory are dynamic in a more essential way, in that a particular change depends not only on the passage of time but on other variables in the system. For example, the rate of increase in the productivity of labor in an industry may depend on an increasing level of production in that industry. Some of these dynamic elements can also be analyzed by methods of comparative statics if our purpose is only to choose among alternative courses of action.

The four assumptions of growth theory discussed above (Section B) lead to the following requirements for the analytical framework to be used in determining comparative advantage in a growing economy:[3] (1) recognition of the possibility of structural disequilibrium in factor markets; (2) the inclusion of indirect (market and nonmarket) effects of expanding a given type of production; (3) simultaneous determination of levels of consumption, imports, and production in interrelated sectors over time when decreasing costs result from the expansion of output; and (4) allowance for variation in the demand for exports and other data over time.

[3] Some of these criticisms of static analysis were made years ago by Williams [66], and a number of the elements were, of course, recognized by the classical economists themselves. I am not concerned with explicit criticism of the classical analysis, but with the possibility of reconciling it with growth theory.

These changes destroy the simplicity of the classical system, in which allocation decisions can be based on a partial analysis because adjustments in the rest of the economy are reflected in equilibrium market prices. In the dynamic analysis, it may not be possible to state that a country has a comparative advantage in producing steel without specifying also the levels of production of iron ore, coal and metal-working over time. In short, we are forced to compare alternative patterns of growth rather than separate sectors, and we cannot expect to find simple generalizations of the Heckscher-Ohlin type concerning the characteristics of individual lines of production.

Since there is no well-developed body of theory concerning the formal properties of the system just outlined,[4] I shall only try to indicate in a general way the modifications that some of these elements of growth theory will produce in the analysis of comparative advantage.

Factor Costs. It is generally agreed that costs of labor and capital in underdeveloped countries do not reflect their opportunity costs with any accuracy because of market imperfections, but there is wide disagreement as to the extent of the typical discrepancies. Some types of labor may be overvalued while particular skills are undervalued. Factor costs may also change markedly over time as a result of economic development, so that an advantage based on cheap labor may prove quite limited in duration. As Lewis [29] and Hagen [21] show, the effects on comparative advantage of correcting for disequilibrium factor prices are often very substantial. (The effects of disequilibrium in factor markets are discussed further in Part II.)

Export Markets. Two of the main arguments against the trade pattern produced by market forces concern (1) the fluctuating nature and (2) the low income and price elasticities of the demand for primary products. The existence of cyclical fluctuation is well established, but the income and price elasticities vary considerably among primary commodities. Their net effect on the terms of trade of primary producers over time is a matter of dispute [64]. These characteristics are often used as an argument for reducing specialization in underdeveloped countries and for expanding industry for local consumption rather than expanding primary exports [41] [51].

These factors can be admitted without seriously modifying the principle of comparative advantage. The market value of the stream of export earnings should be reduced to reflect the drawbacks to the economy resulting from its variable characteristics, and this social value should be used in comparing investment in primary exports to other

[4] In his survey of modern trade theory, Caves [7] shows that attempts to introduce dynamic elements have been concerned mainly with particular aspects and have led not to new principles, but rather to extensions of static results.

alternatives. When export demand has a low elasticity, marginal revenue should be used in place of average revenue. Since it is quite likely that the market evaluation of the attractiveness of an investment in exports will differ from this social evaluation, some form of government intervention may be warranted. It is wrong, however, to conclude from this analysis that continued specialization in primary exports may not be the best policy, because even the corrected return on exports may be greater than that on alternative investments. The supply of foreign investment may also be greater for export production.

Productivity Change. The possibility of rising efficiency as labor and management acquire increasing experience in actual production has long been recognized [66] and forms the basis for the infant industry argument. This argument has been generalized to include the effects of increasing production in any industry on the supply of skilled labor and management available to other industries. Since manufacturing is thought to have more important training effects than primary production [33] [41], the fact that improvements in factor supply are not reflected in the market mechanism may introduce a bias against manufacturing. The empirical basis for this argument has been questioned by several economists [46] [63], who assert that there is often as much scope for technological improvement in agriculture as in industry. Without trying to settle the empirical question that has been raised, it may be concluded that productivity change is an important factor and therefore that comparative advantage should be measured over time. It cannot be said, however, that allowance for this factor will always favor manufacturing.

Dynamic External Economies. As indicated above, dynamic external economies are received by an industry from cost reductions or demand increases in other sectors. Cost reductions may result from economies of scale, productivity increases, or new technology. The customary analysis of comparative advantage on a sector-by-sector basis would require that the cost reduction from simultaneously developing interrelated sectors be allocated separately to each. However, if a group of investments will only be profitable when they are undertaken together, comparative advantage can only be determined for alternative combinations of investments. As shown in [11], not only do market prices fail to produce the best investment allocation in this situation, but any structure of equilibrium prices may also be an inadequate guide in the presence of economies of scale.

There is considerable evidence that external economies are more important in the industrial sectors than in primary production because of internal economies of scale, training effects, and high demand elasticities. Their omission from the market mechanism is therefore likely to

bias resource allocation against manufacturing. The quantitative significance of this factor is very hard to determine, however, since it involves simultaneous changes in a number of sectors.

Uncertainty and Flexibility. The limited ability of policy-makers to foresee changes in demand and supply conditions puts a premium on flexibility in the choice of a development strategy. This factor not only argues against specialization in one or two export commodities but it also favors the development of a diversified economic structure which will enable the economy to shift to new types of exports or import substitutes when changing trade conditions may require them. Kindleberger [26] sees this factor as the main explanation for his finding that the terms of trade have favored developed countries although they have not favored countries exporting manufactured goods in general.[5] The argument is similar to that of Stigler [52] concerning the optimum choice of techniques in a manufacturing plant. The optimum design for a changing market is likely to differ from the optimum under static conditions because in the former case the proper criterion is lowest-cost production for varying operating levels and with changes in product design. Similarly optimum development policy should result in a pattern of resource allocation that allows for unforeseen changes in supply and demand conditions even at the cost of some loss of short-term efficiency.

II. *The Measurement of Optimum Resource Allocation*

The development of an adequate theory is only the first step in formulating economic policy. In order to reach practical conclusions, it is also necessary to specify the environment in which the policy-maker functions. Relevant aspects of a particular society include its general objectives, the policy instruments to be considered, and the information available. The theory must then be combined with these elements in such a way as to yield guides to action or "decision rules" for particular situations.

Although the growing science of operations research is concerned with the development of decision rules for business and military operations, less progress has been made in developing an operational approach to long-run economic policy. Tinbergen [55] and Frisch [15] have outlined a general framework for policy analysis, but it has had relatively little impact on the discussion of the development of underdeveloped countries. In this field the failure to specify adequately the decision-making environment and to distinguish between decision rules and the corollaries of pure theory has led to great confusion.

Since the information needed for over-all economic analysis is avail-

[5] This argument is also discussed by Caves [7, pp. 264-66].

able to a very limited extent in underdeveloped countries, there has been a considerable effort to derive decision rules or "investment criteria" that can be based on partial analysis. I shall group the various suggestions into three categories: (1) factor-intensity criteria; (2) productivity criteria; (3) programming criteria based on accounting prices. Although these various approaches often lead to contradictory results, each has some merit as a form of decision rule if properly qualified. In general, the theoretically more valid formulations require more information and must be replaced by cruder approximations when adequate data are not available. Since a major part of the literature in the development field has been devoted to the discussion of investment criteria, it is important to identify the sources of conflict among them and to specify the circumstances under which each may be approximately correct.

In economic theory, capital and labor are assumed to be separately allocated in single units to different uses. In national planning, however, it is more convenient to consider the decision to install a given productive process or plant, representing the allocation of a group of inputs in specified quantities, as the basic choice. Investment criteria are customarily formulated for "projects" of this sort, since they form the basis for the decisions of planning authorities. This procedure recognizes that very small productive units are uneconomical, and it permits a consideration of different scales of output. The choice of techniques can be considered as a choice among projects producing the same output from different input combinations. In this way the allocation procedure can be divided into two steps: the choice of the best technique for a given type of product, and the decision whether to produce the commodity at all. The principle of comparative advantage is more directly relevant to the second type of choice, but the two cannot be separated entirely.

A. *Factor-Intensity Criteria*

The simplest approach to any allocation problem is to concentrate on the scarcest resource. Since this is often capital in underdeveloped countries, it seems reasonable to choose the technique that uses the least capital to produce a given output. The same logic is applied to the choice of sectors of production: an underdeveloped country is advised to produce and export commodities that use relatively less capital per unit of output and to import items requiring more capital. Statements of this type occur in many economic writings of the past fifteen years. Buchanan [5] was among the first to state this criterion for investment in underdeveloped countries and to base policy recommendations upon it.

The "minimum capital-output ratio" criterion is only valid under the following restrictive conditions:[6] (1) Either capital is the only scarce factor in the system, or other inputs are so abundant relative to capital that the latter is the dominant element in determining cost differences. (2) Either the same output is produced by each investment alternative, or the market values used to compare the different products coincide with their social values. (3) Production takes place under constant costs.

The use of the capital-output ratio theoretically requires a measurement of the total capital used in producing a given commodity, including the capital used in producing all materials and services purchased. Alternatively, the indirect use of capital can be allowed for by deducting the cost of purchased inputs from the value of output and expressing the criterion as the ratio of capital to value added. This procedure requires the further assumption that market prices correctly reflect the use of capital in the rest of the economy.

A closely related allocation criterion is the capital intensity: the ratio of capital to labor. This test is derived directly from the Heckscher-Ohlin version of the comparative cost doctrine. If the same production functions exist in all countries and if capital is scarce relative to labor in the underdeveloped countries, comparative advantage in the latter can be identified by low capital-labor ratios. This approach does not assume that labor has zero opportunity cost, as does use of the capital-output ratio, but only that the ratio of labor cost to capital cost is lower than in the country's trading partners. To allow for differences in the quality of labor among countries, it is sometimes suggested that the assessment of relative labor cost should be made for labor units of equal efficiency—e.g., the labor required in each country to perform a given type of operation with the same capital goods and organization.

A principal criticism of the use of both these ratios is that they ignore the existence of other factors of production, such as natural resources. If either labor or natural resources has a significant opportunity cost, the capital-output measure must be replaced by the more general marginal productivity of capital criterion, which is discussed in the next section.

To judge comparative advantage by the capital-labor ratio is to assume either that this ratio will be the same for the same industry in all countries, or that capital is equally substitutable for labor in producing all the commodities traded. Deviations from these assumptions, along with the omission of other inputs and variations in efficiency by

[6] A rigorous analysis of the validity of marginal and average factor-output ratios as indicators of optimum allocation in a two-factor system is given by Bator [4].

sector, make the capital-labor criterion a very crude approximation indeed to a proper estimate of comparative advantage.

B. *Marginal Productivity Criteria*[7]

A more comprehensive allocation criterion is the social marginal product of a given unit of resources in a given use. Where the factor-intensity criteria are at best only correlated with the increase in national income produced by a project, the productivity criteria try to measure the increase. The marginal productivity test is in turn less general than the over-all programming approach, because it is based on a partial equilibrium analysis that is only valid for relatively small changes in the economic structure.

The several forms of marginal productivity criterion that have been proposed differ in the assumptions made about the social welfare function and in the extent to which allowance is made for the indirect effects of a given allocation. All versions are alike in assuming that the government controls, directly or indirectly, a certain fraction of the investible resources of the country and wishes to allocate them in such a way as to maximize future welfare.

Since the productivity criteria are usually applied to investment projects rather than to single units of capital, they are "marginal" only in the sense that a project normally constitutes a small fraction of the total capital invested in a given year. For very large projects a breakdown into smaller units would be more appropriate.

The Static SMP Criterion. As proposed by Kahn [25], the social marginal product (SMP) is a general equilibrium concept which is conventionally defined as the net contribution of a marginal unit (project) to the national product.[8] The related decision rule is to rank investment projects by their SMP and to go down the list until the funds to be allocated are exhausted. Alternatively, any project having an SMP above a given level can be approved.

Kahn uses the SMP criterion to show the fallacies in the factor-intensity measures that had been advocated by Buchanan [5], Polak [40], and other writers. He points out that: "The existence of a particular natural resource, specialized skills, particular climatic conditions, or the importance of a particular product or service may make the SMP of capital higher in a line which is more capital intensive than in another which is less so" [25, p. 40]. He also argues that even when there is substantial rural unemployment, a considerable amount

[7] Surveys of these and other investment criteria are given by Castellino [6], Vaidyanathan [62], and the United Nations [61].

[8] To be more accurate, cost and output streams should be discounted to the present, but I shall not be concerned with differences in the time pattern of output of different projects.

of capital and other inputs are required to transport, train, and house the workers who are to be employed elsewhere. Kahn's arguments against the simple capital-intensity criteria appear to have been generally accepted, although he admits that a lower capital-output ratio may be a useful guide when other information is lacking.

Some modifications in the SMP criterion were suggested by the present author [8] to allow for artificial elements in the price system (tariffs, subsidies, etc.) and to provide for the evaluation of labor and foreign exchange at opportunity cost rather than at market value. Further allowances for the difference between market price and social value can be made by estimating the benefits to be provided to other sectors in the form of external economies, and by including overhead costs in the estimate of the cost of labor. All of these elements are included in Eckstein's synthesis and extension of the productivity approach [14].[9]

The SMP criterion is entirely consistent with the general programming approach discussed below, which derives opportunity costs from an explicit analysis of total factor use. In the absence of such an overall analysis, the corrections suggested for the calculation of the productivity of investment are likely to be quite approximate. There is no logical conflict between the results of the SMP analysis and the dictates of comparative advantage because each is a corollary of a general equilibrium solution over a given time period.

The Marginal Reinvestment Criterion. A sharp criticism of the SMP criterion was made by Galenson and Leibenstein [17], who challenge some of its basic premises. They would substitute a different social welfare function in which the aim is to maximize per capita income at some time in the distant future rather than to maximize a discounted stream of income over time. They also assume severe restrictions on the policy instruments available to the government, and in particular deny its ability to affect the rate of saving by fiscal measures. Under these assumptions, it is necessary to take account of the division of income resulting from a project between profits and wages, since savings from the former are higher.

To maximize the total output at some distant future time, Galenson and Leibenstein easily show that the most "productive" project is not necessarily the one which maximizes national income in the near future but the one which leads to the highest savings. Since it is assumed that neither voluntary saving nor taxes can be extracted from wages, the most productive project will be the one with the highest profit rate per

[9] Eckstein points out that the assumption of capital rationing implies a social judgment as to both the amount of investment in the current period and the discount to be applied to future outputs, since the market rate of interest is rejected for both purposes.

unit of capital invested.[10] The assumption that profits are saved and reinvested leads to the "marginal reinvestment quotient" as a decision-rule to be applied in place of the SMP.

Galenson and Leibenstein push their argument one step further and identify the most profitable project as the one with the highest capital-labor ratio. This result leads them to the paradoxical conclusion that the factor-intensity rule should be reversed: countries should prefer the most capital-intensive rather than the least capital-intensive techniques in order to promote savings and future growth. This conclusion involves an implicit assumption about the nature of production functions: that increasing the capital intensity will necessarily raise the average return to capital in each sector of production. This is obviously not true in general and is not necessarily true of existing productive techniques. The savings effect of a given project should therefore be measured directly and not assumed to vary in proportion to the capital-labor ratio.

Galenson and Leibenstein have been widely criticized for their extreme assumptions [4] [14] [24] [35], in particular for the use of a social welfare function in which the starvation of half the population in the near future would appear to be a matter of indifference and for the assumption that limitations on fiscal policy make a lower income preferable to a much higher one if the former has a higher savings component. Their analysis has nevertheless been useful in emphasizing that other effects of an investment beside its immediate contribution to the national product should be included in the productivity criterion.[11]

The Marginal Growth Contribution. Eckstein [14] has successfully reconciled the conflict between the Kahn-Chenery SMP approach and the Galenson-Leibenstein reinvestment approach, and in so doing he has provided a considerable generalization of each. First, he assumes that the social objective is to maximize the present value of the future consumption stream. With a zero discount rate, this objective approximates the long-term income objective of Galenson and Leibenstein, while with a high discount of future consumption it leads to the maximization of income in the short term. Second, Eckstein assumes that there is a different savings (reinvestment) coefficient associated with each project, but he allows for any savings rate out of wages and profits. From these assumptions, he derives a measure of the "marginal growth contribution" of a given project that consists of two parts: (1)

[10] I omit the possibility of an effect on population growth, which leads Galenson and Leibenstein to state the criterion on a per capita basis.

[11] In [28], Leibenstein restates in more restrained form his arguments for including labor training, savings, population growth, and other indirect effects in a comprehensive productivity measure.

an *efficiency term*, consisting of the present value of the consumption stream; and (2) a *growth term*, consisting of the additional consumption to be achieved by reinvesting savings.

The relative importance of the two terms depends largely on the rate of discount that is applied to future consumption. Even with a low rate of discount, the significance of the second term depends on how much variation there is in the fraction of income saved among different projects. If the savings ratio is not related to the form of income generated, then, as Bator [4] shows, there is no conflict between maximizing income in the short run and in the longer run. Eckstein's formula provides for all possible intermediate assumptions between the two extreme views of the determinants of savings.[13]

In principle, one might include other indirect dynamic effects, such as the value of the labor training provided, in the measurement of the total productivity of a given project. There is a danger of double counting if partial-equilibrium analysis is extended too far, however, and most indirect effects can be more readily evaluated in the more general programming framework considered below.

C. *Programming Criteria and Accounting Prices*

The allocation rules discussed up to now are based on the existing economic structure and are strictly applicable only for relatively small changes in it. Although it may in many instances be necessary to rely primarily on these marginal criteria for lack of data on the rest of the economy, it is important to have some way of testing larger changes and of evaluating the errors that are introduced by the marginal procedure. Furthermore, without a more comprehensive analysis it is impossible to reconcile fully the conflicting policy implications of comparative advantage and growth theory.

The difficulties of partial analysis increase with the number of modifications that have to be applied to market prices in order to arrive at social value. Both the factor-intensity ratios and the partial productivity measures assume that there is one principal restriction on the system, the scarcity of capital. They do not allow for the fact that in allocating capital according to any one of these rules some other restriction on the system, such as the supply of foreign exchange, of skilled labor, or of a particular commodity, may be exceeded.

The programming approach to resource allocation begins with the problem of balancing supply and demand for different commodities and factors of production. Until quite recently, practical programming

[13] Sen [49] independently formulated a more general investment criterion that is very similar to Eckstein's, in which the SMP and reinvestment criteria are shown to be limiting cases.

methods have been more concerned with ensuring the consistency of a given allocation of resources with certain targets than with testing the efficiency with which resources are used. Historically speaking, the programming approach is thus the operational counterpart of the theory of balanced growth, from which much of its conceptual framework is derived.

One of the earliest attempts at formulating a comprehensive development program for an underdeveloped area was Mandelbaum's illustrative model for Southeastern Europe, undertaken during the war [31]. He starts, as many subsequent programs have done, from an estimate of the increase in national income required to absorb a prospective increment in the labor force. The allocation of capital and labor is made initially from demand estimates and by analogy to the structure of more advanced countries. The principle of comparative advantage is only introduced intuitively in modifying the initial projection. The main test of resource allocation is the balance of demand and supply for each sector and factor of production.

The development of mathematical programming methods makes it possible to carry out this type of analysis in a much more precise way. In several countries, consistent development programs have been formulated by using input-output analysis, as in the studies of the Economic Commission for Latin America [58] [59] [60]. It is only with the development of linear programming, however, that it is possible to reconcile the consistency criteria and the productivity criteria in a systematic way.

A link between the test of consistency (feasibility) in resource allocation and the test of productivity (efficiency) is provided by a consideration of the price implications of a given allocation. Assume that a set of production levels has been worked out so as to be consistent with the available supplies of labor, capital and natural resources, given the structure of consumer demand and the country's trading possibilities. These sector production and trade levels constitute a "feasible program." Any such program implies a unique set of commodity and factor prices if the economy is in equilibrium. If production activities are assumed to operate at constant costs, linear programming provides a method of calculating the "shadow prices" corresponding to the equilibrium conditions, in which the price of each commodity is equal to its cost of production.[13] Prices are determined by the solution to the following set of simultaneous equations, one for each production activity included in the program:

$$(1) \qquad a_{1j}P_1 + a_{2j}P_2 + \cdots + a_{nj}P_n = 0 \qquad (j = 1 \cdots n)$$

[13] The assumptions of linear programming and methods of finding solutions to programming models have been discussed in a number of recent publications, such as [13].

where a_{ij} is the input or output of commodity or factor i by activity j, and P_i is the shadow price of commodity or factor i. The input coefficients may be measured at existing prices or in other convenient units. In an open economy, activities of importing and exporting are also included in the system, and the price solution contains the equilibrium price of foreign exchange. An example of this calculation is given in Table 1, which will be explained shortly.

The use of shadow or "accounting" prices in evaluating investment projects has been suggested by Tinbergen [54] [56], Frisch [15] [16], and Chenery [9] [10]. Although Tinbergen does not use a linear programming framework, his accounting prices for factors have the same meaning as shadow prices: the opportunity cost implied by a given resource allocation.[14] He suggests computing the costs associated with a project by using accounting prices; any project that shows a positive net return over cost (including capital cost) should be approved. This test is equivalent to the SMP criterion, as shown below.

The general linear programming problem is to maximize the value of a linear objective function subject to linear constraints. In development programs, the principal constraints are that the demands for commodities and factors should not exceed their supplies; the function to be maximized is usually taken as the national income. Alternatively, the objective may be the achievement of a given increase in output at minimum cost in investment (including foreign investment). Other social objectives, such as a minimum employment level or a specified degree of regional balance, can be included as additional restrictions on the program. The instrument variables can also be constrained to fall within specified limits, as in the models of Frisch.[15]

To illustrate the meaning and use of shadow prices in evaluating investment projects, I shall take up a very simplified programming model that is worked out in more detail elsewhere [11]. The truncated system given in Table 1 covers only a small part of the economy, but it will serve to illustrate the way in which interdependence influences in-

[14] Tinbergen [56, p. 39] defines accounting prices as those "that would prevail if (i) the investment pattern under discussion were actually carried out, and (ii) equilibrium existed on the markets just mentioned" [i.e., labor, capital, foreign exchange markets]. The relation between accounting and shadow prices is discussed in Chenery [10] and Qayum [42].

[15] Frisch is one of the strongest advocates of the use of linear programming for development planning, as indicated in the preface to a recent methodological study: "In the beginning of 1959, during my work as a United Nations expert in Cairo, I was confronted with the problem of working out a methodology for *optimal investment programming* in a rapidly expanding underdeveloped country. I have always believed—and my Cairo experiences have confirmed it—that such a method must be formulated in terms which ultimately make the problem amenable to linear programming. Otherwise one is practically certain to be taken by surprise afterwards in unexpected balance of payments difficulties and other troubles" [16, p. 1].

TABLE 1—EVALUATION OF PRODUCTION AND IMPORT ACTIVITIES BY ACCOUNTING PRICES[a]

Commodities and Factors	Production Activities				Import Activities			Accounting Prices				Restrictions (12)
	X_1 (1)	X_2 (2)	X_3 (3)	X_4 (4)	M_1 (5)	M_2 (6)	M_3 (7)	Trial a (8)	Trial b (9)	Trial c (10)	Trial d (11)	
1. Metal Products	1.00 (3.41)				1.00 (3.41)			2.55	3.42	3.41	2.26	1000
2. Iron and Steel	− .22 (− .89)	1.00 (4.03)				1.00 (4.03)		3.60	4.82	4.03	3.50	1000
3. Iron Ore		− .08 (− .25)	1.00 (3.12)				1.00 (3.12)	3.30	4.42	3.12	2.19	0
4. Foreign Exchange				1.00 (4.01)	− .85 (−3.41)	−1.20 (−4.81)	−1.10 (−4.41)	3.00	4.02	4.01	2.92	0
5. Other Inputs	− .20 (− .62)	− .25 (− .78)	− .70 (−2.17)	− .10 (− .31)				3.00	3.20	3.10	2.20	—
6. Labor	− .70 (−1.05)	− .20 (− .30)	− .30 (− .45)	−1.00 (−1.50)				1.50	1.50	1.50	.50	—
7. Capital	− .70 (− .70)	−2.70 (−2.70)	− .50 (− .50)	−2.20 (−2.20)				1.00	1.00	1.00	1.00	—
Social Profitability[b]												
Trial a	− .59	− .41	+ .25	−1.00	0	0	0					
Trial b	− .03	+ .37	+1.23	0	0	0	0					
Trial c	+ .15	0	0	0	0	− .78	−1.29					
Trial d	0	− .03	0	0	− .22	0	−1.02					
Production and Import Levels												
Trial a	0	0	0	2050	1000	1000	0					
Trial b	0	1000	80	850	1000	0	0					
Trial c	1000	1220	98	0	0	0	0					
Trial d	1000	0	0	1464	0	1220	0					

[a] Based on Chenery [11], Table 1. Prices satisfy equation (1) except for P_4 in trial 1. Figures in parentheses are $(a_{ij} P_i)$ for trial c.
[b] Calculated from equation (4).

vestment decisions and the effect of having more than one scarce factor.

The model contains four production activities (X_1, X_2, X_3, X_4) and three import activities (M_1, M_2, M_3). Each activity is represented in Table 1 by a column of coefficients, a_{ij}, showing the amount of input (−) or output (+) of commodity i when the activity is operated at unit level. (These coefficients are the boldface figures in columns 1 to 7.) The net output is taken as unity in all cases. The production activity X_1, for example, represents the production of one unit of metal products from .22 units of iron and steel, .20 units of "other inputs," .70 units of labor, and .70 units of capital. The import activity M_1 provides an alternative way of supplying a unit of metal products by an expenditure (input) of .85 units of foreign exchange. A similar choice is provided between X_2 and M_2 (iron and steel) and between X_3 and M_3 (iron ore). The fourth production activity shows the resources used in the marginal export sector to provide a unit of foreign exchange.

In a complete programming model, the amounts of all commodities required for final use at a given level of income would be entered as restrictions on the solution. Similarly, the amounts of available capital and labor of different types would be specified. In this limited illustration, the problem is to supply requirements of 1000 each for metal products and iron and steel at minimum cost. Iron ore and foreign exchange are therefore taken to be intermediate goods having no net outside demand. "Other inputs," labor and capital are supplied from outside the model at prices reflecting their opportunity costs in the rest of the economy. The main difference in principle between this submodel and a complete programming system is that the prices of only the first four commodities are determined in the model in the present case, while in general all prices are so determined.

The four restrictions in the model consist of equations stating that the supply of each of the first four inputs must be equal to the specified demand:[18]

$$\begin{aligned} X_1 + M_1 &= 1000 \\ -.22X_1 + X_2 + M_2 &= 1000 \\ -.08X_2 + X_3 + M_3 &= 0 \\ X_4 - .85M_1 - 1.20M_2 - 1.10M_3 &= 0 \end{aligned} \tag{2}$$

The objective is to minimize the amount of capital required to supply the given final demands, with the use of labor and "other inputs" valued at their opportunity costs in terms of capital. This is the same

[18] I omit the possibility of overfulfilling demands, since there are no joint products in the present case.

as supplying each commodity at minimum unit cost, since the amount of each to be supplied is fixed.

A feasible solution to the model contains either a production or an import activity for each of the three commodities plus the export activity for foreign exchange. The corresponding activity levels can be determined from equations (2) and are shown at the bottom of Table 1. The amounts of the outside factors (F_i)—labor, capital, and "other inputs"—required by each solution can then be determined from the following equations:

$$\text{(3)}\quad \begin{array}{ll} \text{Other inputs:} & F_5 = .20X_1 + .25X_2 + .70X_3 + .10X_4 \\ \text{Labor:} & F_6 = .70X_1 + .20X_2 + .30X_3 + 1.00X_4 \\ \text{Capital:} & F_7 = .70X_1 + 2.70X_2 + .50X_3 + 2.20X_4 \end{array}$$

The programming model thus contains two types of equations: price equations of the type of (1), and equations for the supply and demand of commodities and outside factors, (2) and (3). As outlined in [10], the general procedure for solving a programming model of this type involves three steps: (a) finding a feasible program or set of activity levels that satisfies the supply-demand restrictions; (b) calculating the shadow prices associated with the given program; (c) using these prices to determine whether any improvement in the initial program is possible. This procedure is repeated as long as any further improvements can be made.

The programming criterion used to compare projects or activities is the social profitability of each as measured from the shadow prices. Any profitable activity should be included in the program. It is the recalculation of prices that distinguishes this procedure from the partial programming approach suggested by Tinbergen. In either case, however, the test of social profitability of activity j can be expressed as:

$$\text{(4)}\qquad \Pi_j = \sum_i a_{ij}P_i$$

By definition, the activities that were used in determining the shadow prices will have a profitability of zero. The optimum solution is identified by the condition that all other activities have zero or negative profitability.

Some idea of the type of adjustment that results from moving from partial toward general equilibrium analysis may be given by determining solutions to the model in Table 1 under four different procedures: (a) the use of market prices; (b) correcting for the overvaluation of foreign exchange; (c) finding the optimum solution for the submodel alone; (d) finding the optimum solution for the submodel with changes in the opportunity costs of labor and other inputs determined from a

general programming model. The accounting prices corresponding to each assumption are shown in columns 8 to 11 of Table 1. The calculation of social profitability of each activity, given the accounting prices, is illustrated in the table for trial c by giving cost and revenue figures in parentheses in columns 1 to 7.

Trial a. Assume that market prices are based on the cost of importing and are determined by setting profits on the import activities equal to zero, with a given foreign exchange cost of 3.00. The exchange rate is assumed to be overvalued, so that the price of foreign exchange is less than the cost of securing it through expanded exports. At these market prices, only activity X_3 (iron ore) is profitable, but there is no domestic demand for iron ore unless steel is also produced (the export price is lower than that of imports because of transport costs). The use of market prices therefore leads to imports of steel and metal products, since the opportunity cost of expanding exports is not taken into account. The corresponding activity levels are shown at the bottom of the table.

Trial b. Assume now that we correct for the existing structural disequilibrium by setting the price of foreign exchange equal to its opportunity cost of 4.02 as determined from the export activity X_4. Allowance is also made for a rise in the accounting price of "other inputs," some of which are imported. A new set of accounting prices for commodities 1-3 is determined from the cost of imports. Substituting these prices into equation (4) shows that X_2 and X_3 are both profitable ($\pi_2 = .37$, $\pi_3 = 1.23$). Investment should therefore take place in steel, iron ore, and exports on this test.

Trial c. To find the optimum solution to the submodel by linear programming, we can start from trial b and recalculate the shadow prices from the activities that are included: X_2 X_3 X_4 M_1. The four shadow prices P_1 to P_4 are determined by applying equation (1), taking the prices of the outside inputs (P_5, P_6, P_7) as given. The elimination of excess profits from the prices of iron ore and steel lowers the cost of producing metal products, providing an example of pecuniary external economies. Instead of a loss, activity X_1 now shows a profit of .15 and should be substituted for the import activity M_1. With the original prices for labor and capital, the optimum solution to the submodel is therefore to produce all three commodities and import nothing, since all import activities are unprofitable.

Trial d. If a similar analysis is carried out for the economy as a whole, it is likely that the initial estimate of the opportunity cost of labor (equal to its market price) will be revised. Assume that the shadow price of labor (equal to its marginal product in the rest of the

economy) is only a third of its market price, or .5 units of capital. This lower labor cost will reduce the costs of production in different activities in proportion to their use of labor. Since exports are cheapened more than steel production by this calculation, it now becomes socially profitable to import steel and produce metal products. The optimality of this solution is shown by the prices in trial d, in which there is a loss of $-.03$ on X_8. The optimum quantity solution is shown at the bottom of the table. Valuing other inputs and labor at their accounting prices, it has a capital cost of 5760, compared to 8200, 7470, and 7290 in trials a, b, and c.

The programming approach of trials c and d adds two elements to the analysis of accounting prices. The first is the inclusion of repercussions on input prices from investment in supplying sectors. This is one of the main types of dynamic external economies which are omitted from partial analysis. It is much more significant when there are economies of scale. The second element is the revision of the initial estimate of the opportunity costs of labor, capital, and foreign exchange. This revision is determined by the relation between supply and demand for these factors and thus takes into account the requirements of feasibility.[17]

The profitability criterion (usually called the "simplex" criterion) that is used in linear programming is logically equivalent to the SMP test if the same prices are used in both. The two can be put in a comparable form as follows:

(4a) Social profit on activity j: $$\Pi_j = \sum_i a_{ij}P_i - k_j$$

(5) SMP of investment in activity j: $$(\text{SMP})_j = \frac{\sum_i a_{ij}P_i}{k_j} = \frac{\Pi_j}{k_j} + 1$$

where $-k_j$ is used for the capital input coefficient instead of a_{7j}. An activity having a positive social profit in equation (4a) will have an SMP of greater than 1.0 in (5), and the same projects would be accepted by either test. If the prices used are not the equilibrium prices, however, the project rankings by the two formulae will not necessarily be the same.

Although the example given here contained only one technique of production for each commodity, linear programming methods readily encompass alternative techniques. In a trial application of linear programming to Indian planning, Sandee [45] includes three alternative

[17] An example in which these successive adjustments are calculated in detail is given in [10]. Frisch has outlined a computational procedure for handling large numbers of investment projects without going beyond the capacity of simple calculating equipment [16].

ways of increasing agricultural output—increased use of fertilizer, irrigation, and extension services—which are substitutes over a limited range. The four alternative techniques for producing textiles cited by Galenson and Leibenstein [17] could also be more properly evaluated in a programming model in which the cost variation associated with their different requirements for materials, maintenance, and skilled labor could be included. However, it is only necessary to include alternative techniques in a programming model when the choice between them depends on the outcome of the solution. Probably in most cases the range of shadow prices can be foreseen accurately enough to determine in advance which technique is more efficient for a given country. The initial assumption can always be verified after the analysis has been completed by using the resulting prices.

Linear programming can be extended to include many of the indirect effects of investment that are suggested by growth theory. The production of trained labor, the effect on savings, or other indirect benefits can be considered as joint outputs whose value can be specified in the objective function. Similarly, indirect costs of production, such as the provision of housing to urban workers, can be included as additional inputs. The shadow prices computed from such an expanded system will therefore reflect nonmarket as well as market interdependence to the extent that it can be specified in quantitative form.

In formal terms, it is also quite easy to extend the programming model in time and to compute future prices for commodities and factors. The measurement of social profitability could then be made against a pattern of changing future prices. Given the degree of uncertainty attached to all future economic magnitudes, however, this is not likely to be a very useful procedure beyond the customary five-year planning period except in the most general terms. It would, however, be desirable to estimate the change in the equilibrium prices of foreign exchange and labor over a longer period of time, since these are the most important variables in choosing among investment projects.

D. *Investment Criteria and Comparative Advantage*

The linear programming approach provides a convenient link to the principle of comparative advantage because the optimal pattern of trade is determined simultaneously with the optimum allocation of investment. The model is considerably more general than that of market equilibrium because it allows for different social objectives and takes account of costs and benefits other than those entering the market. The limitations to the programming model are of two sorts: the form of the restrictions that are specified, and the omission of relationships that cannot be expressed in quantitative form.

The introduction of inelastic demands or increasing costs does not create any more theoretical difficulty in a programming model than in the corresponding general equilibrium system, although the computational aspects of such models have not been widely explored. The accounting prices perform the same function as guides to proper allocation, but the test of social profitability must be applied in marginal rather than average terms. In development programs, this modification is particularly important in the case of exports, where the price elasticity of demand is often rather low.[18] As Nurkse [37] points out, marginal comparative advantage for the underdeveloped countries may for this reason be quite different from that inferred from the average costs and prices of primary exports.

The existence of increasing returns creates the same problem for the programming model as it does for equilibrium theory. Marginal-cost pricing is not sufficient to determine whether an investment should be undertaken, and the total cost of alternative solutions must also be considered. Although practical methods of solving programming models containing decreasing costs are now being developed, they do not give allocation criteria that rely only on accounting prices. It is approximately correct to say that beyond a certain output level country A has a comparative advantage in the production of steel, but the precise determination of the break-even point depends on the level of output in other sectors also.[19]

The most serious theoretical qualification to the principle of comparative advantage comes from the type of nonquantitative interdependence among sectors that is assumed by Hirschman [23]. If, as he supposes, one growth sequence is more effective than another because it economizes on decision-making ability or provides a greater incentive to political action, a set of criteria having little or nothing to do with comparative advantage is implied. The empirical significance of these psychological and sociological factors remains to be established, but they lead to a conflict that cannot be resolved in economic terms.

When the practical limitations on information and analysis are recognized, the possibilities of conflict between comparative advantage and growth theory are greatly increased, and Wiles [65] suggests that marginal efficiency calculations may be less important. An aversion to risk-taking may be a valid reason for limiting the extent of specialization in the export of primary products beyond the amount that would be optimum in the light of more accurate information. An inability to

[18] A programming model including this feature is given in Chenery [9].

[19] The nature of solutions to this type of problem is considered in [11], from which the data in Table 1 were taken. In this situation of decreasing average cost, the programming model may provide a greater improvement over the solution using partial criteria.

measure the extent of economies of scale, labor training, and other sources of external economies also makes possible a continuing disagreement as to their magnitude.

III. *Comparative Advantage and Balance in Development Programs*

The inconsistent procedures that governments employ in formulating development policies are probably the most important source of conflict between the dictates of comparative advantage and of growth theory. Official pronouncements on development policy usually allege that both types of criteria have been (or should be) utilized in drawing up the program that is put forward, but the procedure followed in reconciling conflicts between the two is rarely made explicit. Since the analytical basis of most development programs is quite limited, it is important to look into the procedure that is actually used in order to discover sources of bias.

Development programs must simultaneously confront two sets of problems. In the short run, progress is hampered by structural disequilibrium in factor markets and in the demand and supply of particular commodities. This disequilibrium is reflected in the balance-of-payments difficulties that beset most low-income countries as they try to accelerate the process of development. In the longer run, the choice among sectors becomes increasingly important because the pattern of growth in each period will depend on the choices made previously. Development programs that are influenced mainly by the existing structural disequilibrium therefore tend to stress the need for greater balance between domestic demand and supply, while those that take a longer view tend to pay more attention to comparative advantage.

Although the procedures actually followed cannot be ascertained with any accuracy by an outside observer, these two aspects can be identified from characteristic elements in the analysis. The balanced growth approach is generally associated with target-setting in key sectors, stress on the avoidance of bottlenecks, and attempts to equate the supply and demand of labor, capital, and the more important commodities. The extreme cases of this type of procedure are found in the communist countries. Less extreme examples, in which some attention is paid to comparative advantage, are the procedures of the Indian Planning Commission and the U.N. Economic Commission for Latin America.

Characteristic elements of the comparative advantage approach are attempts to measure the relative efficiency of different types of production, the weighing of balance-of-payments improvements against other benefits to the economy (by means of accounting prices or otherwise), and usually a greater emphasis on partial analysis than on over-all

projections. Examples that will be cited are Puerto Rico, the Philippines, and Israel.

A. *Procedures Emphasizing Domestic Balance*

The planning procedures developed in the USSR and applied with some modification in other communist countries represent in extreme form the use of balance as a criterion for resource allocation and the virtually complete omission of any test of comparative advantage. As revealed in recent studies by Montias [32] and Balassa [1], the main tool of Soviet-type planning is a very detailed system of material balances specified in quantitative terms. Policy objectives are translated into production targets in which priority is given to heavy industry and other sectors that are expected to contribute to further growth ("leading links"). Prices are used mainly as rationing devices and have no necessary connection with production costs. The cumbersome calculations involved in arriving at balance of supply and demand for a large number of commodities limit the alternatives that can be tried out, so the main effort is to find a feasible program [32].

The question of comparative advantage scarcely arises in the USSR because of its size and diversified resources, although similar problems arise in connection with the choice of production techniques. When the Soviet planning system was transplanted to the satellite countries, however, it ran into difficulties because of its inability to determine the advantages to be secured from trade. According to Balassa [1, p. 264], the idea of comparative advantage did not exist in Hungarian development policy (at least until very recently) although trade has a high ratio to GNP. Exports are determined by import "needs," and the institutional structure is such as to encourage exporters to meet targets for exports without regard to production costs. Since prices do not reflect resource use, it is impossible to determine where comparative advantage lies and to what extent the trade pattern deviates from the optimum.

Despite their violation of most short-term welfare considerations, the success of Soviet planning methods in producing a rapid rise in the national product makes them attractive to many underdeveloped countries. In India, for example, Mahalanobis' "plan-frame" for the second five-year plan [30] draws heavily on Soviet methodology. He starts from the assumption that the rate of investment is determined by the level of domestic production of capital goods: "As the capacity to manufacture both heavy and light machinery and other capital goods increases, the capacity to invest (by using home-produced capital goods) would also increase steadily, and India would become more and more independent of the import of foreign machinery and capital

goods" [30, p. 18]. His analysis implies that export possibilities are so limited that they can be ignored, so that the composition of demand is limited by the composition of domestic output. In order to raise the level of investment, Mahalanobis concludes that investment in industries producing capital goods should be increased from less than 10 per cent to 30-35 per cent of total investment in the second five-year plan.

As Komiya [27] has shown, Mahalanobis' approach to development ignores price and demand considerations completely. The targets for the four sectors in his model appear to be based mainly on the goal of creating heavy industry, which is assumed to be the key to future growth. Criteria of efficiency and comparative advantage are entirely omitted from his analysis.

Although there are traces of the Mahalanobis approach in the second and third five-year plans formulated by the Indian Planning Commission, the final results are much less extreme. One basic problem is that exports are expected to rise only half as fast as national income between the first and third plan periods, while demand for the goods initially imported tends to rise much more rapidly. The inelastic demand for traditional Indian exports means that a considerable proportion of investment must be devoted to commodities that are presently imported. Within this category, the principles of comparative advantage should apply. In actuality, the emphasis has shifted somewhat from heavy industry in the second plan to agriculture in the third. In the latter document [19], increasing self-sufficiency in basic industrial commodities—steel, petroleum, machinery, etc.—is listed as a high-priority objective, but so is the maximum development of agriculture. Whether the resulting targets are consistent with comparative advantage is not considered in the published analysis.[20]

The balance-of-payments difficulties of many Latin American countries have also been a major factor in shaping the programming procedure developed by the Economic Commission for Latin America [57]. This approach has been applied in considerable detail in studies of Colombia [58], Argentina [59], and Peru [60]. One basic conclusion of these studies is that the growth of exports will be much slower than the growth of demand for goods that are currently imported. Investment therefore has to be heavily oriented toward import substitution, and the equality of supply and demand must be tested on a commodity basis to avoid balance-of-payments difficulties. In the three cases mentioned, this balancing process is carried out by means of an input-

[20] On the basis of a simplified linear-programming model, Sandee [45, p. 25] finds that "up to 1970 more effective ways to employ capital for development exist than highly capital intensive steel-making," suggesting that an analysis of comparative advantage would indicate more reliance on imports. The nonmarket benefits of production are omitted from his analysis, however.

output analysis in which imported goods are distinguished from domestic products in each category.

In principle, comparative advantage can be used in the ECLA procedure as a basis for the choice of import substitutes, but this has apparently been done only to a limited degree. Since the main emphasis is on balance, there is a danger that the initial assumptions as to levels of exports will not be re-examined after the extent of import substitution required by a given program has been determined. The result may be a considerably lower productivity of investment in import substitutes than in exports if the two are not systematically compared. The drawbacks to this procedure are more serious in small countries like Colombia and Peru than in a large country like India, in which imports supply a smaller fraction of the total demand for commodities.

B. *Procedures Emphasizing Comparative Advantage*

Among countries having development programs, procedures that stress comparative advantage are less common than those emphasizing balance. Practically all policy statements list among their priority criteria factors presumably leading to comparative advantage, but there is little evidence as to how they are applied in drawing up programs.

The development procedures of the government of Puerto Rico come as close to being a pure application of comparative advantage as Soviet procedures are of principles of balanced growth. Unlike many low-income countries, Puerto Rico has an elastic demand for its exports to the U.S. market and can attract U.S. capital for profitable investments. The government's policy has been to give tax remission for ten years and to provide overhead facilities, labor training, and other inducements to industries that will benefit the island's economy. In deciding which industries to promote, the Economic Development Authority has studied the long-term comparative advantage of a large number of alternative projects, since comparative advantage will lead to both satisfactory profits and maximum income. Low-cost labor (even with allowance for differences in productivity) has been the main element in comparative advantage, since most industrial materials must be imported. Allowance is also made for external economies in industries that will supply inputs to other sectors.[21]

Under this policy, the growth of per capita income has been as rapid (nearly 5 per cent annually) and the development of industry as marked (from 19 per cent to 25 per cent of GNP) over the years 1948-1958 as in any country following a deliberate policy of balanced

[21] The Puerto Rican experience is discussed by Baer [2]; the evaluation procedures are described in mimeographed reports of the Economic Development Authority.

growth. The planning procedure depends very largely on the particular relation of Puerto Rico to the United States and its small size. These factors make it unnecessary to worry about the elasticity of demand for exports or the dangers of dependence on foreign sources for essential imports, which so preoccupy the Indian and Latin American planners. With reliable export and import markets, domestic balance is not a problem.

Since the assumptions of the classical model are not approached so closely in most underdeveloped countries as in Puerto Rico, the calculation of comparative advantage usually departs farther from the market evaluation. In a more typical case the Philippine National Economic Council has outlined a procedure for applying the SMP formula under Philippine conditions [39]. This analysis starts from the market evaluation of the profitability of an investment and adds corrections for the project's effect on the balance of payments, its use of domestic materials, and its use of domestic labor, each with a suitable weight. This procedure may be justified by comparison to the linear programming criterion of social profit. In principle the proper correction to private profit is obtained by giving each a value equal to the difference between its shadow price and its market price.[22] In the Philippines, this would mean a bonus for labor and a penalty for foreign exchange use (or a bonus for foreign exchange saving). Higgins [22, pp. 654-62] shows that the weights assigned in the Philippines tend to exaggerate these effects. The use of the same weight for all domestic materials may lead to serious error, since not all are overvalued by market prices.

The government of Israel has developed one of the most systematic procedures for measuring comparative advantage as a basis for allocating investment funds and foreign exchange. In effect, the Ministry of Finance evaluates projects on the basis of accounting prices for foreign exchange and capital, taking into account the indirect use of foreign exchange in sectors supplying inputs such as power or industrial materials. The calculation is summed up as the cost in domestic resources of a dollar earned or saved, and it is applied equally to exports and to import substitutes. The calculation of domestic value added is also made by exporters as a basis for export subsidies [3, p. 23]. In allocating the government's development budget, priority is given to projects

[22] The social profit, Π_j, may be expressed as:

$$\Pi_j = \bar{\Pi}_j + \sum a_{ij}\Delta P_i, \tag{4b}$$

where $\bar{\Pi}_j$ is private profit per unit of output calculated at market prices and ΔP_i is the difference between the market price and shadow price of commodity i. The elements ΔP_i may be regarded as weights attached to each input or output coefficient.

whose domestic cost of earning or saving foreign exchange is less than the current estimate of its accounting price. This procedure can also be rationalized by means of the linear programming criterion of social profitability. Instead of measuring the value derived per unit of investment with accounting prices for foreign exchange and labor, as in the SMP formula, the cost per unit of foreign exchange acquired is computed using an accounting price for capital. When the same shadow prices are used, all three measures give the same result.

Although it is dangerous to generalize from the limited evidence on development policies that is available, there appears to be some relation between the type of procedure adopted and the characteristics of the economy in a number of the cases examined. Small countries are forced to pay more attention to comparative advantage because they cannot hope to produce the whole range of manufactures and primary products, while large countries may be tempted to follow more autarchic policies.[23] The importance given to balanced growth also depends to a large extent on the country's recent experience with its export markets and the state of its foreign exchange reserves and borrowing capacity. Puerto Rico and Israel can both count on substantial capital inflows which make it unnecessary for them to approach balanced trade in the near future, while India has much less leeway.

IV. *Conclusions*

This paper has considered development policy from the standpoint of economic theory, as a problem in operations research, and as it is actually carried on by governments. Much of the confusion in the field stems from a failure to distinguish these different levels of analysis. Theorists are prone to suggest decision rules that omit some of the relevant institutional limits, while economists who have been working in particular areas often arrive at conclusions that do not fit other cases. As in other fields of economics, most of the disagreement can be traced to implicit differences in assumptions.

There are a number of contradictions between the implications of trade theory and growth theory. To make the two theories consistent, it is necessary to discard the assumption of equilibrium in factor markets, to allow for changes in the quantity and quality of factors of production over time, and to take account of internal and external economies of scale. Although under these assumptions market forces do not necessarily lead to efficient resource allocation, a pattern of production and trade can be determined that maximizes income over time. The commodities to be produced and traded cannot be determined by a

[23] Japan is one exception to this generalization, partly due to its dependence on imported raw materials.

simple ranking procedure along the lines of classical comparative advantage because of the interdepenence among sectors. At best, it may be possible to say, for example, that a country has a comparative advantage in steel production for a specified set of production levels in supplying and using sectors. In advanced countries, this qualification may be unimportant, but in the less developed ones it is crucial in a number of industries.

Much of the attack on the use of comparative advantage is based on its omission of various nonmarket elements. It is assumed that the inclusion of the latter favors the development of industry, and special benefits are often attributed to capital goods and heavy industry. The intangible benefits stemming from trade in the form of new products, improved technology, and technical assistance tend to be overlooked in this discussion. Although I support the critics who wish to include more of growth theory in determining the desirability of specialization, I doubt that this extension will favor balanced growth to the extent that they suppose.

The other main theoretical attack on comparative advantage is aimed at its supposed support for continued specialization in primary exports. Granting the low elasticity of demand for many primary products, it is wrong to conclude that comparative advantage is thereby superseded by principles of balanced growth. The increasing shortage of foreign exchange makes it even more important to economize on its use and to seek efficient ways for increasing its supply. The comparison of domestic to foreign sources of supply that is implied by comparative advantage is no less relevant to this situation than to the case in which investment is more evenly divided between exports and import substitutes.

The aspects of growth theory which do not seem to be reconcilable with the notion of comparative advantage are the sociological and political effects of choosing one production pattern instead of another. While the concept of opportunity cost can be extended to include a number of nonmarket phenomena, such as labor training and overhead facilities, it can hardly be stretched to cover differences in fertility rates or political attitudes. So far as I can see, in the present state of knowledge of social phenomena, considerations such as these may be used to modify the results of economic analysis but cannot be directly incorporated into it.

At the level of operations research, the search for simple decision rules for investment in low-income countries seems to have been useful mainly in exposing the fallacies in some of the common rules of thumb. One can specify conditions under which ratios such as the capital intensity or the effect on the balance of payments would be a valid indi-

cator of the desirability of an investment, but the apparent gain in simplicity is offset by the danger of applying the test in inappropriate circumstances. A more fruitful approach to partial equilibrium analysis is provided by the use of accounting prices to compute the social profitability of a given use of resources. This method allows simultaneously for several overvalued or undervalued inputs, and it can include whatever elements of general equilibrium analysis are available.

Since market forces cannot be relied on to balance supply and demand under conditions of initial disequilibrium and accelerated growth, a principal concern of development policy is to ensure the consistency of production levels with commodity demands and factor supplies. The technique of linear programming is designed to combine the test of consistency with the test of the social profitability of a given resource use. Although it cannot be applied very extensively in underdeveloped countries as yet, the programming methodology serves as a guide to improved practical measures.

To most economists, a survey of the procedures actually followed in designing development policy would probably suggest that balance is overemphasized and that the potential gains from trade are often neglected. This emphasis may be partly justified by the greater uncertainties attached to trade and by an aversion to risk that is greater than seems warranted to the outside observer. Better understanding of the working of the underdeveloped economies and better information for planning is needed to redress the balance and enable countries to secure the potential gains from trade without conflict with measures for domestic development.

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THE DIFFERENTIAL EFFECTS OF TIGHT MONEY

By G. L. Bach and C. J. Huizenga*

Restrictive monetary policy is widely opposed because of its alleged undesirably discriminatory effects. Tight money, it is claimed, lets big borrowers go free while shutting off little ones. It restricts construction activity while letting investment in plant and equipment boom. Conversely, it restricts investment so sharply it induces recession. It runs up interest costs to those least able to pay. It penalizes new borrowers at the expense of old established customers. All these claims, and many more, have been urged upon Congress, by economists and by others, as powerful reasons against reliance on restrictive monetary policy to check moderate inflation.

Given substantially full employment, any restrictive policy is discriminatory in the sense that it charges the allocation of resources from what would have prevailed in the absence of the restriction. Assume full employment with excess demand (inflationary pressure) and some given allocation of resources. If monetary policy is now used to produce a smaller money supply than otherwise would have existed, a different allocation of resources may result. It is this shift in resources which is presumably meant when critics speak of the discriminatory (or differential) effects of tight money. We shall use the term in this sense.

The following pages describe an investigation of the "discriminatory" effects of tight money which isolates these effects by studying the differential lending-investing policies during the 1955-57 period of "tight" banks in contrast to those of "loose" banks which were otherwise substantially identical but where there was little or no pressure of tight money.

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I. *Design of Study*

Identification of possible discriminatory effects of tight money during any period of credit restraint is difficult. In 1955-57, for example, we know that commercial bank lending to large borrowers rose much more than that to small borrowers. But this fact is not necessarily evidence that tight money led banks to discriminate against small borrowers. Instead, the observed results may have arisen largely from the demand side of markets rather than from the supply side, and indeed there is much evidence that such was the case in that particular period. The problem is to devise a method of isolating the supply effects (that is, the discriminatory effects of tight money in restricting lending) as distinct from the effects of differing demands for credit.

To isolate the effects of tight money on the behavior of lenders, the following basic design was used. First a period was chosen when money was generally agreed to be tight and growing tighter—October 1955 to October 1957. Then a large sample of banks (about 1700) was chosen, large enough to permit stratification so that substantial numbers of banks in all major cells were presumably substantially identical in all respects (including potential loan demand) except for the differential impact of tight money upon them. Then the banks were divided into three subgroups—"tight," "medium," and "loose," depending on the degree of tightness induced in them by the over-all tightness of money. The tightest quartile of banks was placed in the tight group, the next two quartiles in the medium group, and the loosest quartile in the loose group. The loose banks, as is explained below, were selected so that it would be agreed that they were loose by almost any reasonable test—for example, they were not tight by standard tests at the beginning of the period, and they gained more deposits over the period than they increased their loans and investments.[1]

Then the lending and investing behavior of these three groups of banks was compared over the period, with the presumption that the tight quartile would reflect the differential impact of tight money on the supply side, when compared with the loose quartile which apparently felt little if any pressure of tightness. This comparison between the tight and loose quartiles seems especially apt to isolate the differential effects of tight money, since loose banks were clearly quite loose and there is little evidence that they refused any borrowers because of shortage of lending power or for any reason other than failure of borrowers to meet general banking standards of credit-worthiness.

[1] The terms tight, medium and loose are intended as brief terms to indicate relative status. They are not intended to convey absolute status with any precision, except, as is noted below, that the loose banks were demonstrably loose by almost any reasonable standard.

In testing different hypotheses about possible discriminatory effects of tight money, banks were stratified by size and other major characteristics within each of the three tightness groupings, to assure comparability on factors other than tightness.

A. *Nature of Sample and Information Obtained*

The basic sample consisted of about 1700 Federal Reserve member banks, with identical banks reporting in October 1955 and October 1957. Reporting banks held nearly 90 per cent of all commercial and industrial loans at member banks. The sample provided almost complete coverage of all central reserve city and reserve city banks, with about one-fourth of all country member banks. The sample was drawn on a stratified basis by the Federal Reserve System for its two major studies of commercial and industrial loans in 1955 and 1957. All sample data were then "blown up" to cover all commercial member banks in the United States.[9]

Information in both years was collected on the following items: (1) complete call report data for each reporting bank, including information on all major asset and liability items; (2) the following information on individual commercial and industrial loans on the books of each reporting bank as of October 5, 1955 and October 16, 1957: (a) business of borrower (13 categories); (b) total assets of borrower; (c) form of business organization—incorporated or unincorporated; (d) amount of loan outstanding; (e) original amount of loan; (f) whether loan was a term loan; (g) whether loan was secured or unsecured; (h) interest rate on loan.

B. *Measures of Bank Tightness*

For explaining banker (lender) behavior, how tight a bank is depends on how tight the banker (the decision-maker) feels it is. One bank may be extremely tight for lending purposes, even though it has a large volume of excess reserves and liquid securities, *if* the banker believes that these reserves and securities are essential to the sound operation of the bank. Another bank may be loose for lending purposes, even though it has very small excess reserves and only a modest supply of liquid securities, *if* the banker feels that he nevertheless has more reserves and more securities than he needs for normal operating purposes (assuming that he is within standard examination regulations). Thus, standard measures like excess reserves and free reserves are not reliable measures of bank tightness for lending purposes.

This point becomes clearer if one remembers that the individual

[9] Details of the sampling procedure and the reporting forms were published in the *Federal Reserve Bulletin* [9, 10].

banker can alter his volume of excess reserves (and hence his lending power) with relative freedom by restructuring his asset portfolio—say by selling off bills or bonds. Thus one must consider the whole asset portfolio—not just a simple measure of excess or free reserves—if he is to have a reasonable measure of how tight the individual bank is. And the banking system as a whole can similarly increase its excess reserves by selling securities to others, though to a lesser extent since it must find noncommercial-bank buyers, a limitation which the individual bank does not face.

This poses difficult problems of measuring the tightness of individual banks and of the banking system. We cannot peer into the banker's mind to see what makes him feel tight or loose. Indeed, the banker's own word is possibly not to be accepted. So we need to search for surrogate measures.

Banking System as a Whole. Over the period from October 1955 to October 1957, it is widely agreed that money was tight and becoming increasingly tighter for the banking system as a whole.[3] At least four types of evidence support this belief.

First, Federal Reserve authorities, bankers, and virtually all observers in the financial press spoke out on the increasing tightness of money. While such statements are of course not conclusive, their general uniformity was striking.[4]

Second, over the period commercial banks shifted heavily out of long-term bonds into short-term government securities and loans. Between October 1955 and October 1957, loans at all members banks increased from $67 billion to $80 billion while bonds of 5 years or longer maturity declined from $24 billion to $10 billion. This shift was a clear indication of increasing pressure on the banking system so far as the ability to make loans was concerned.

Third, interest rates had risen substantially by the beginning of the period, and continued to rise through it, as is indicated by Table 1.

Fourth, there was virtually no growth in the money supply, although the volume of transactions to be financed and population rose substantially over the period. Currency and demand deposits outside banks totaled $132 billion in October 1955, and only $134 billion in October 1957. At the same time gross national product rose from $392 billion

[3] The exact dates chosen (in October of each year) were dictated by the availability of data—both call-report data and, more important, data on the large-scale Federal Reserve commercial loan surveys which were available for only those two specific months. Actually, a period ending a few months earlier, in the summer of 1957, would have been better, since apparently the peak of tight money occurred some time in the late summer. However, there was no substantial easing of money over the few months before October.

[4] See, for example, the annual reports of the Board of Governors of the Federal Reserve System [7]; "Bank Credit and Money" in [5] [6]; the *New York Times* financial pages [12]; and *Business Week* [8].

TABLE 1—INTEREST RATES, OCTOBER 1955-OCTOBER 1957

Average for 1954		October 1955	October 1957
U. S. Treasury bills	.9	2.2	3.6
Prime commercial paper	1.6	2.7	4.1
Aaa corporate bonds	2.9	3.1	4.1

(annual rate) for the third quarter of 1955 to $440 billion (annual rate) for the third quarter of 1957.[8]

Clearly, there have been other periods when money was tighter, and in part the increasing tightness was a return to more normal times from the very low interest rates of the preceding decades. For purposes of this study, however, it is important merely that money was tight enough to put the tighter banks under substantial pressure to refuse some otherwise acceptable borrowers, and that it was becoming tighter. These conditions were clearly present. Nor do the findings depend on the extent to which this tightness reflected conscious Federal Reserve policy. Since the money supply remained roughly constant, the increasing tightness obviously reflected mainly increasing demand for money.

Individual Banks. To test tight money hypotheses, we ranked all individual banks by degree of tightness as of October 1955, and by increase in tightness between October 1955 and October 1957. A more satisfactory measure than excess or free reserves appeared to be the ratio:

$$\frac{\text{excess reserves} - \text{borrowing} + \text{government bills and certificates}}{\text{deposits}}$$

We call this a looseness ratio, since an increase in the ratio means that the bank has become looser for lending purposes.

This ratio was used to rank individual banks as of October 1955. The ratio reflects the fact that banks consider short-term governments

[8] The traditional measures of excess reserves and "free" reserves provide little help in assessing the tightness of the banking system over the period in question. Excess reserves averaged about $500 million during October of each year. This reflected the fact that excess reserves were substantially at their operating minimum by 1955, given the mores of many bankers about excess reserves. Thus they could not practically be reduced further. Free reserves (excess reserves minus borrowing) averaged —$360 million in October 1955 and —$344 million in October 1957. Banks that were willing to borrow at the Federal Reserve were doing so substantially by October 1955, and again to about the same extent in October 1957. Both the free and the excess reserve figures emphasize that many banks nowadays manage their portfolios so as to hold excess and free reserves at what they consider reasonable minimum levels, especially when interest rates are high. Thus, whether money is loose or tight, excess reserves for the system stay at about the same level. Free reserves are more volatile and are significant for many large banks. But they too provide a very imperfect measure of the tightness of the system, for the reasons noted above and because only a small fraction of banks view borrowing at the Federal Reserve as a significant device for adjusting their reserve positions.

as secondary reserves, only slightly differentiated from actual reserves. Moreover, this ratio varies appreciably at individual banks with changes in economic conditions, at the same time that the ratio of excess reserves, or even free reserves, to deposits varies little for most banks. The ratio falls (indicates tightening) for the banking system as a whole and for most individual banks over the 1955-57 period, when we know that money was tightening for the system as a whole. On the other hand, the ratio has weaknesses. For example, it does not reflect the fact that interbank deposits provide a special source of liquidity to some banks; thus, most small country banks were probably relatively looser than the ratio shows. Neither is vault cash included. Nor are near-maturity securities other than bills and certificates. Most important, it does not include longer-term government securities, but there are convincing reasons for this exclusion.[8]

We have no clean-cut objective basis for selecting the looseness ratio used. The case is that it is a reasonable measure a priori, and that all the likely alternatives have serious drawbacks. The ratio was tested against other measures, including excess and free reserves. For example, the ratio of loans to government securities was examined, on the theory that the higher the loan ratio becomes the tighter the bank will be since it has less opportunity left to shift from government securities to loans. This measure, like the looseness ratio including government bonds, proved of limited usefulness because it mainly reflected the lending-investment preferences of individual banks, rather than serving as a fundamental measure of tightness for the rank-ordering of banks.

To measure the *change* in tightness between October 1955 and October 1957 two tests were initially applied. First, all individual banks were ranked by the decrease in the looseness ratio between October 1955 and October 1957. Second, banks were ranked according to the percentage increase in their deposits over the period. For the individual bank, as distinguished from the banking system, it is primarily gain or loss of deposits which makes the bank looser or tighter for new lending and investing. Therefore, the simplest measure of whether an individual bank is growing looser or tighter is the extent to which it is gaining or losing deposits. Thus, all banks were ranked by percentage increase

[8] Government bonds, which are not included in the numerator, obviously help increase liquidity and hence decrease the tightness of a bank. While individual banks can obtain funds for loans by selling government securities, holdings of long-term governments at many banks are so large relative to bills, certificates, and free reserves, that their inclusion would swamp the ratio. Thus the ratio with long-term government securities included would tend to reflect primarily the investment preferences of individual banks and would lose most of its virtue as a measure of tightness for ranking individual banks.

To avoid the danger that the deposits and reserve figures in the ratio would be thrown off by special temporary factors, monthly averages were used, rather than one-day figures.

in deposits over the two-year period. Banks with the greatest loss of deposits showed the greatest increase in tightness, with others ranked in order of deposit gain.

Broadly, the rank-order results for individual banks were similar using these two methods over the 1955-57 period. However, the change-in-deposits method both seemed more significant in explaining individual bank lending-investment behavior and offered a more sharply discriminating measure as among individual banks. This is because changes in the tightness ratio were quite small for most banks, so that the individual bank ranking might be considerably influenced by small special circumstances, while differences in the rate of deposit growth were large. Thus, we decided to use the second measure alone—change in deposits between October 1955 and October 1957—as the criterion of the extent to which banks became tighter or looser.[7]

To obtain the final tightness ranking of all individual banks, the ranking as of 1955 and the ranking by increase in tightness for the 1955-57 period were combined in the following way. First, banks were divided into the tightest and loosest halves on the basis of the looseness ratio as of October 1955. Then, all banks in the tightest half for 1955 were rank-ordered by the degree to which their tightness increased over the succeeding two years, as measured by relative deposit loss or gain. The tight group for the study (the tightest quartile) was then obtained by taking the 50 per cent of the tight half as of 1955 which showed the greatest further increase in tightness by 1957. Similarly, the loosest half as of 1955 was rank-ordered by change in tightness, and the 50 per cent showing the greatest increase in looseness was considered the loose group for the study. The remaining two inner quartiles were considered the medium group.[8]

This test combines tightness as of the beginning of the period with change in tightness. In principle, there need be no relationship between these two measures. On the other hand, the purpose was to segregate at the two extremes banks which both were tight in absolute level and became tighter, from those that were clearly loose in absolute level and became looser. The procedure followed achieved this result. Thus, banks in the loose quartile had looseness ratios of 3 per cent and higher in October 1955, as compared to only 1+ per cent for all banks. More-

[7] A further study was made to test the significance of using both measures. Limitation of the tight group to banks that were in the tightest quartile by *both* the change-in-looseness ratio and the change-in-deposits tests eliminated only a small fraction of the banks. This further refinement was therefore dropped.

[8] Since large city banks were heavily concentrated in the tight group, about 40 per cent of total commercial bank assets were included in that group. About 45 per cent were in the medium group, and about 15 per cent in the loose group in which smaller country banks predominated.

over, their gain in deposits ranged from 8 per cent to over 100 per cent for the two-year period, compared to only a 4.5 per cent increase for the banking system as a whole—while about half the banks in the tight group actually lost deposits over the two-year period.[9] Most important, the loose banks as a group gained more deposits over the period than they expanded their loans and investments. Thus, they obtained more new funds for loans and investments than they used. Under this circumstance it is hard to see how these banks can have felt themselves seriously restrained by tight money.[10]

C. *Hypotheses Investigated*

Using this analytical approach, five general hypotheses were considered: (1) That tight money induced banks to shift from government securities to loans. (2) That tight money led banks to discriminate against small borrowers in lending to businesses. (3) That tight money led banks to differentiate in favor of particular industry groups among business borrowers. (4) That tight money was effective in checking loans especially to those firms which were primarily responsible for the 1955-57 investment and inventory boom. (5) That tight money led banks to raise interest charges especially to small borrowers and to particular industry groups against which they wished to discriminate. The succeeding sections examine these hypotheses in turn.

II. *Effects on Bank Lending and Investing*

Table 2 compares the behavior of tight, medium, and loose banks in extending loans and investments over the 1955-57 period as money grew tighter. The left-hand portion of the table shows the percentage increases of total loans and investments and all major subclasses at loose, medium, and tight banks. Percentage increase figures are used because absolute figures would overweight the large banks in whatever groups they fell (largely the tight and medium groups). The right-hand portion of the table shows the relative increases (or decreases) in loans and investments at loose, medium, and tight banks. Though only relative changes are shown, the absolute amounts in all cells are large.

[9] Studies were made of the differences in groupings obtained by using either the as-of-1955 or the 1955-57 change measure alone. Surprisingly, not very great changes were obtained in the tight and loose groups by limiting the test to the situation as of October 1955 or by taking the change-in-deposit ranking alone. Thus, it appears that, in a broad sense, the banks that were already tight in late 1955 were the ones that tended to become even tighter over the following two years.

[10] This same excess of new deposits over new loans and investments was shown by all small (country) banks as a group. There was a massive shift of deposits (and lending power) from very large to small banks. See [11, p. 424].

TABLE 2—INCREASE IN ASSET CLASSES, OCTOBER 1955–OCTOBER 1957

Asset Groups	Per Cent Increase at:			Relative Increase, with Per Cent Increase at Loose Banks = 100		
	Loose Banks	Medium Banks	Tight Banks	Loose Banks	Medium Banks	Tight Banks
Total Loans and Investments	*23*	*9*	*1*	*100*	*39*	*4*
Bills and certificates	87	85	242	100	98	278
Other government securities under 5 years	36	26	12	100	72	33
Government securities over 5 years	−49	−52	−52	(a)	(a)	(a)
Other securities	34	5	−10	100	15	−129
Commercial and industrial loans	47	33	25	100	70	53
Real estate loans	32	16	6	100	50	19
Security loans	154	22	−20	100	14	−13
Agricultural loans	4	3	−10	100	75	−250
Loans to individuals	30	24	11	100	80	37

a Decrease in all groups.

As the left-hand portion shows, all banks increased their total loans and investments, but the loose banks did so the most. All banks sold off long-term government securities, presumably to obtain funds to increase other assets. All banks increased their holdings of short-term government securities and of commercial and industrial loans, loans to individuals, and real estate loans. The large percentage increase in short-term government securities of all banks, however, is caused in substantial part merely by long-term bonds moving down into the under-5 years category, rather than by actual bank sales of long-term bonds and purchase of short-term issues. If adjustment is made for this moving down in issues held, it is still true for all banks combined that there was some shift from long- to short-term government securities, but not much. This qualification does not, so far as we can tell, throw doubt on the *differential* behavior shown by loose and tight banks.[11]

But there were appreciable differences in the behavior of tight and loose banks, as indicated by both halves of the table. Tight banks substantially reduced their holdings of other securities and of agricultural and security loans, while building up their short-term government securities more heavily than other banks. Their increase in loans was smaller than at other banks, and the differences in lending-investing

[11] This result seems surprising, since bankers are generally thought to draw first on short-term government securities to obtain loan funds when reserves become tight. Once adjustment is made for the downshifting of maturities, the actual dollar increase in bills and certificates at tight banks in Table 2 is small, but not insignificant. In any case, it is clear that tight banks drew most heavily on longer-term securities to obtain loan funds.

behavior at tight and loose banks were greatest within the loan categories. Tight banks increased real estate loans much less than did loose banks. But still more, they squeezed security and agricultural loans heavily to obtain funds for modest expansions in other loan categories. On the other hand, security and agricultural loans have never been dominant parts of the loan portfolio of the banking system, and the actual dollar shift of loans was more modest than might appear from the relative increases.[12]

It may be surprising that the tight banks did not shift *more* heavily from low- to high-yield assets under the pressure of tight money. The explanation is probably found largely in the force of traditional standards of banking practice. Most bankers, even when very tight, are reluctant to go beyond certain widespread notions of portfolio balance, which vary substantially by class and location of bank. For example, loans amounting to much more than 50 per cent of total assets apparently seem excessive, or at least of dubious propriety, to many bankers. Moreover, bankers understand their needs for liquidity and do not consider loans very liquid, in spite of the technical availability of the Federal Reserve rediscount window. Federal Reserve informal and formal actions reinforce this reluctance to rely extensively on rediscounting except in special temporary circumstances. Thus, many bankers continue to be the generally careful, cautious people they are commonly reputed to be in determining their portfolio balance, even when profits beckon in, say, higher automobile or real estate loans.[13]

If we assume that loose banks felt little or no restraint from tight money (as is strongly suggested by the evidence on pages 58-59), then the

[12] It might appear that this differential behavior of tight and loose banks is explained not by differing tightness, but merely by the fact that the expected mean value of the lending-investing behavior of the two groups is similar so that they will both tend to move toward it—the so-called "regression fallacy." In Table 2, the greater shift from government securities to loans at loose banks might simply represent a movement of the loose and tight banks back toward a common portfolio balance after the tight group had by chance increased their loans more rapidly. But examination of the nine asset categories in Table 2 shows disparate behavior that is not explained by the regression fallacy. While we cannot be sure that the observed lending-investing differences between tight and loose banks are explained by differing tightness, the behavior is generally consistent with what we would a priori expect to observe from the tight-money hypothesis; and we find no other reasonable hypothesis to which the observed behavior can be attributed.

[13] For a summary of banker interviews on the extent to which tight money changed lending policies, see [11, p. 431 ff.]

Apparently bank examination standards per se did not significantly limit bank loan expansion during the period. In an unpublished doctoral dissertation at Carnegie Institute of Technology, David Chambers found that even tight banks (using our groupings) generally stayed well within the formal examiners' limits. Other tests confirmed this general conclusion. But widespread knowledge of examiners' expectations, of course, may have helped mold bankers' mores as to how far they can reasonably go in shifting to loans, and to higher-yield risky loans within the loan category, when money becomes tight.

Table 3—Increase in Assets at Banks of Different Sizes, October 1955–October 1957

Assets at Banks of Different Sizes[a]	Per Cent Increase at:			Relative Increase, with Per Cent Increase at Loose Banks=100		
	Loose Banks	Medium Banks	Tight Banks	Loose Banks	Medium Banks	Tight Banks
Total Loans and Investments						
All banks	*23*	*9*	*1*	*100*	*39*	*4*
Under $10 million	31	10	1	100	32	3
$10–100 million	19	4	4	100	21	21
$100–1,000 million	23	13	0	100	54	0
Over $1 billion	(b)	8	1		100	15
Bills and Certificates						
All banks	*87*	*85*	*242*	*100*	*98*	*278*
Under $10 million	127	91	938	100	72	739
$10–100 million	78	86	211	100	110	271
$100–1,000 million	43	91	338	100	212	790
Over $1 billion	(b)	66	85		100	129
Other Government Securities under 5 Years						
All banks	*36*	*26*	*12*	*100*	*72*	*46*
Under $10 million	41	19	9	100	46	22
$10–100 million	32	25	27	100	78	85
$100–1,000 million	38	26	11	100	68	29
Over $1 billion	(b)	35	7		100	19
Government Securities over 5 Years						
All banks	*−49*	*−52*	*−52*			
Under $10 million	−48	−48	−45			
$10–100 million	−51	−52	−55	(c)	(c)	(c)
$100–1,000 million	−47	−51	−50			
Over $1 billion	(b)	−56	−52			
Other Securities						
All banks	*34*	*5*	*−10*	*100*	*15*	*−29*
Under $10 million	55	17	3	100	31	1
$10–100 million	27	12	7	100	44	26
$100–1,000 million	23	5	−18	100	21	−76
Over $1 billion	(b)	−8	−17		(c)	(c)
Commercial and Industrial Loans						
All banks	*47*	*33*	*25*	*100*	*70*	*53*
Under $10 million	68	18	4	100	26	6
$10–100 million	36	24	16	100	67	44
$100–1,000 million	51	31	19	100	60	37
Over $1 billion	(b)	47	30		100	64

[a] All categories by bank size are based on deposits as of October, 1955.
[b] No banks over $1 billion deposits in the loose category.
[c] Decrease in all groups.

TABLE 3—*Continued*

Real Estate Loans						
All banks	*32*	*16*	*6*	*100*	*50*	*19*
Under $10 million	35	19	13	100	54	37
$10–100 million	31	13	8	100	42	26
$100–1,000 million	23	20	6	100	84	25
Over $1 billion	([b])	14	3		100	22
Security Loans						
All banks	*154*	*22*	*−20*	*100*	*14*	*−113*
Under $10 million	1248	542	948	100	43	76
$10–100 million	13	68	16	100	523	123
$100–1,000 million	54	− 1	− 1	100	− 3	− 2
Over $1 billion	([b])	1	−32		100	−467
Agricultural Loans						
All banks	*4*	*3*	*−10*	*100*	*75*	*−250*
Under $10 million	2	8	− 5	100	400	−250
$10–100 million	8	−27	9	100	−338	113
$100–1,000 million	13	22	−25	100	175	−202
Over $1 billion	([b])	1	−77		100	−1316
Loans to Individuals						
All banks	*30*	*24*	*11*	*100*	*80*	*37*
Under $10 million	28	22	14	100	79	50
$10–100 million	25	21	14	100	84	56
$100–1,000 million	42	31	11	100	75	27
Over $1 billion	([b])	18	7		100	39

comparative data for tight banks provide a direct measure of the differential impact of tight money. Even if the loose banks felt some restraint, since the tight banks clearly were much tighter the comparative data still provide direct evidence on the "discriminatory" effects of tight money on bank lending and investing behavior.

Attributing the differences in Table 2 to tight money implies that banks of comparable size in the three groups were substantially identical on other grounds, particularly in the loan demands they felt. We believe this was substantially true.[14] The 1700 banks in the sample, as indicated above, provide substantially complete coverage of large- and medium-sized banks; and the sample of small banks was carefully stratified geographically and in terms of other significant bank characteristics. Lending-investing behavior varied at banks of different

[14] This is, of course, a crucial assumption. Otherwise, observed differences between the behavior of tight and loose banks cannot necessarily be attributed primarily to differences in tightness. We can only report that, in addition to the careful sampling procedure followed, we have examined the bank groups in detail for other characteristics that might explain a significant part of the observed differences, and have been unable to find any—for example, geographical or urban vs. country location. It is important to remember, however, that separate analysis of banks of different sizes is important at several points because of the relative concentration of large, city banks in the tight group and small, country banks in the loose group.

sizes. Table 3 provides complete data, comparable to Table 2 above, for banks of different sizes.

Another possible objection to this interpretive pattern is that tight money may have driven some borrowers away from tight banks, but that these borrowers readily obtained the desired loans at loose banks (which were under little restraint), so the apparent differential effects at tight banks were just offset at loose banks. This hypothesis depends on the assumption of high mobility of borrowers between tight and loose banks. While some such mobility certainly existed, it was far from perfect. For large borrowers, loose banks of adequate size to make large loans were very scarce; there were no banks of over $500 million deposits in the loose category. For smaller borrowers geographical mobility is limited, and even within given areas small firms find it harder to move readily from one bank to another for credit. It seems unlikely that the apparent impact of tight money at tight banks was completely, or even substantially, offset by shifts to loose banks.[15]

III. *Discrimination by Size of Business Borrower*

One of the commonest objections to the use of tight money to check moderate inflation is that this policy discriminates against small businesses. During the 1955-57 period as shown in Table 4, loans to big businesses did indeed expand much more than those to small businesses. This does not, however, necessarily mean that tight money led to discrimination against small borrowers. Instead, the pattern of loans may have reflected differing demands from large and small borrowers, where the loan demands of credit-worthy large borrowers (as judged by commercial banking credit standards) rose more rapidly than those from credit-worthy small borrowers.

In fact, the recent major Federal Reserve study of lending to small business arrives at this conclusion. This study found that most bankers were ready and willing to lend to small businesses whenever small businesses met normal standards of credit-worthiness. The demand for bank credit rose much less rapidly at small businesses between 1955 and 1957 than at large businesses, and the study reports that this was the main apparent reason for the differential growth in lending. Little evidence was found of discrimination against small borrowers, except in so far as refusal of loans because of inability to meet traditional banking credit standards is considered discrimination. But even here, there was little evidence of a substantial increase in potential small borrowers turned away over the period of tight money.[16]

[15] For an analysis of the effect of monetary restraint on different sectors of the economy, which includes noncommercial bank lenders, see W. L. Smith [4, pp. 362-94].

[16] For summaries of the evidence on a variety of tests, see especially [11, pp. 368-69,

TABLE 4—BANK LOANS TO BUSINESSES[a]

Asset Size of Borrower[b] (000's omitted)	Per Cent Increase in Loans October 1955–October 1957
All borrowers	*31.9*
Less than $50	− 3.0
$50 to $250	16.7
$250 to $1,000	24.8
$1,000 to $5,000	21.3
$5,000 to $25,000	24.7
$25,000 to $100,000	51.1
$100,000 or more	66.4

[a] Reproduced from [11, p. 37]. Data cover commercial and industrial loans at all member banks, plus real estate loans to businesses.
[b] As of October 1955.

While the evidence generally fails to support the hypothesis that tight money leads banks to discriminate against small business borrowers, the argument has not been unmistakably refuted. We therefore conducted the following test of the hypothesis. The same groupings of banks into tight, medium, and loose were continued. To improve comparability banks were further divided into five different size-groups (based on volume of deposits). For this and all succeeding analyses of business loans, data include all commercial and industrial loans plus real estate loans to businesses at all member banks. The increase in loans to borrowers of different sizes was compared at tight, loose and medium banks, both for all banks combined and for banks in each of the five size-groups. If tight banks increased loans relatively more to large (compared to small) borrowers than did comparable loose banks, this test says that tight banks discriminated against small borrowers. Since the demand for loans was presumably substantially identical at tight and loose banks within bank size-groups and since loose banks were not restrained significantly by tight money, the analysis presumes that any such discrimination by tight banks would be attributable to tight money.

Table 5, for example, shows that at medium-sized banks loans to borrowers of all sizes rose more at loose than at tight banks, with the behavior of medium banks intermediate. We might say that tight banks discriminated against borrowers of all sizes, but they surely did not

374-81, 427-31, and 436-39]. The entire Part II of this volume, prepared by the Federal Reserve staff, provides a well-rounded analysis of the total problem of possible discrimination against small borrowers; it concludes that most evidence fails to support this criticism of tight money. A strong statement of the counterview is presented by J. K. Galbraith [1]; but without extensive empirical data to support his argument. Data contradicting the Galbraith argument are presented by Allan Meltzer [2].

TABLE 5—INCREASE IN LOANS TO BUSINESS BORROWERS AT MEDIUM-SIZED BANKS, OCTOBER 1955–OCTOBER 1957[a]

Assets of Borrower (000's omitted)	Per Cent Increase in Loans at:		
	Loose Banks	Medium Banks	Tight Banks
Under $50	21	−11	−13
$50-250	76	10	5
$250-1,000	72	25	25
$1,000-5,000	72	50	30
$5,000-25,000	90	49	30
$25,000-100,000	266	104	14
$100,000 and over	25	30	22

[a] Commercial and industrial loans plus real estate loans to businesses at all member banks with total deposits of $100–500 million as of October 1955.

discriminate especially against small borrowers. On the contrary, compared to loose banks, they discriminated especially against most *large* borrowers. That is, loose banks increased their loans to large borrowers by percentages far in excess of the increases of loans to small borrowers, while tight banks increased their loans to large borrowers only somewhat more than to small borrowers. Since borrower loan-demand was presumably substantially identical at loose, medium, and tight banks, this evidence appears, at least for these medium-sized banks, clearly to reject the hypothesis that tight money led banks to discriminate especially against small borrowers.[17]

Figures 1 through 6 are intended to facilitate examination of comparative increases in loans to different sized borrowers at loose, medium, and tight banks. Figure 1 shows the data for the entire banking system; the others show the data for banks in five different size groups. When the curves slope upward, large borrowers received larger percentage increases in loans than did small borrowers over the two-year period. When the curves slope downward, the reverse was true. Least-squares lines have been fitted to facilitate these visual comparisons. For example, Figure 4 shows the same data as are presented in Table 5 above.[18]

In Figure 1, for all banks combined, the upward slopes of the curves for tight, medium and loose banks are very similar, indicating similar

[17] In Table 5, as in Table 4, the fact that loans rose more to large than to small borrowers does not necessarily indicate discrimination against small borrowers, because the observed differences may reflect primarily differences in loan demand from different sized borrowers. Only a test like that in the text to eliminate possible demand differences can isolate possible lender discrimination.

[18] In Figure 1, total business loans in 1957 to all borrowers were $40.8 billion. Loans to borrowers with assets under $50,000 were $1.5 billion; those to each other size group of borrower shown in Figure 1 ranged from about $5 billion to $8.8 billion.

treatment of small and large borrowers by all three groups of banks. The tight-bank least-squares line slopes upward slightly more than the other two, reflecting primarily as is explained below, the behavior of banks in the $1000-$1,000 million deposits size-class. But we in-

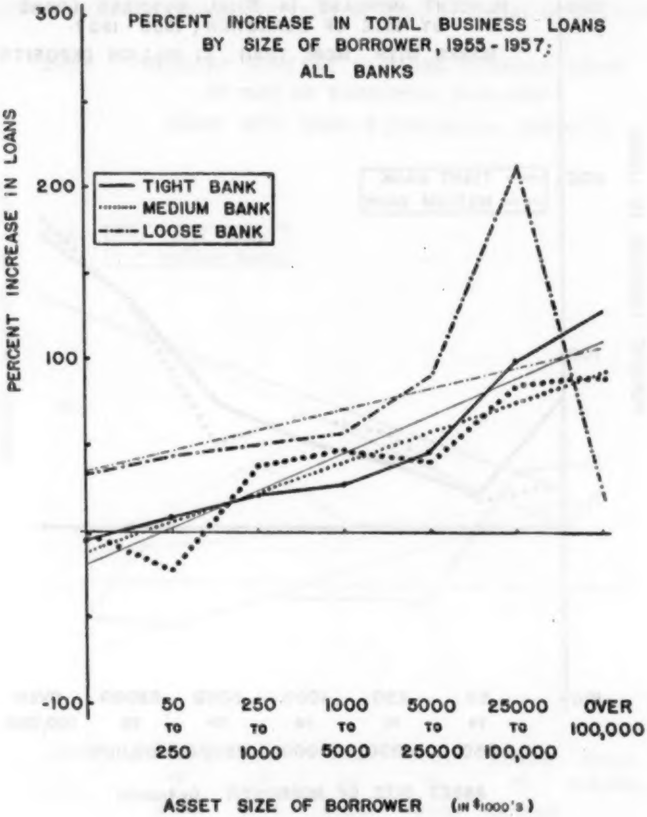

FIGURE 1

terpret the data as substantially rejecting the hypothesis that tight money led banks to discriminate especially against small business borrowers. Special allowance must be made for a crucial point on the loose-bank curve which is based on inadequate data,[19] and the charts for the different bank size-groups strengthen this interpretation.

[19] The final point on the loose-bank curve (loans to borrowers with over $100,000,000 assets) pulls the loose-bank least-squares line down substantially. Since nearly all banks big enough to have such large borrowers were in the tight and medium groups, this particular point is based on a small number of relatively small loans, and has very limited

Figures 2 and 3 show the behavior of very large and large banks (over $500 million deposits), which included no loose banks. In this comparison between tight and medium banks, tight banks in the $500-$1,000 million deposit class did discriminate more against small bor-

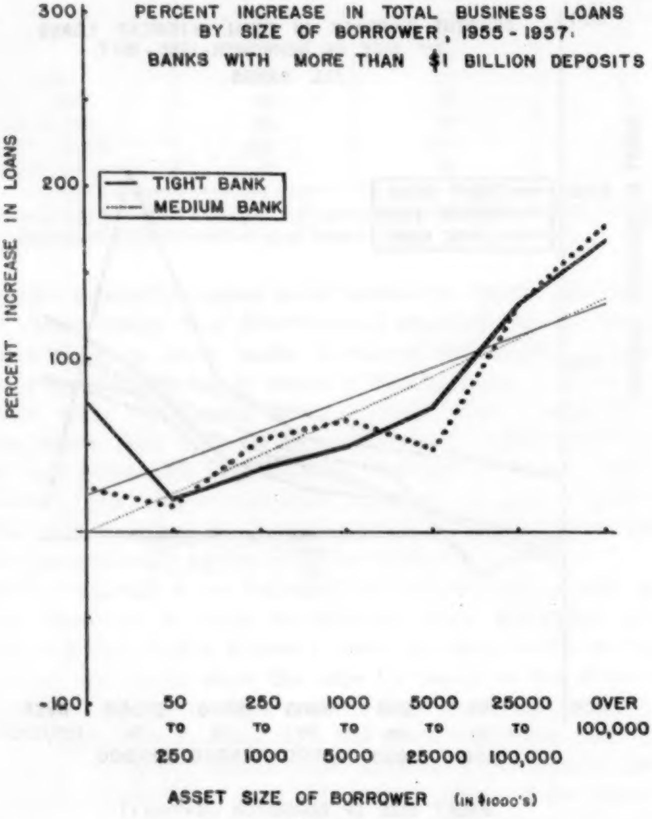

FIGURE 2

rowers than did medium banks of the same size. But Figures 4, 5 and 6 show no such discrimination at other banks where tight and loose banks could be compared directly. On the contrary, at these banks, tight money led to discrimination especially in favor of smaller borrowers.

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significance. A least-squares fit omitting this one point would give a loose-bank line rising more sharply than the tight-bank line, and would thus remove the small amount of all-bank evidence appearing to support the hypothesis of discrimination against small borrowers.

Similar comparisons of loans by tight, medium and loose banks to different sized borrowers were made, breaking businesses into 13 different industry groups—five groups in manufacturing and mining, plus wholesale trade, retail trade, commodity dealers, sales finance companies, public utilities, construction, real estate, and services. The comparisons indicate a wide diversity of lending behavior to borrowers

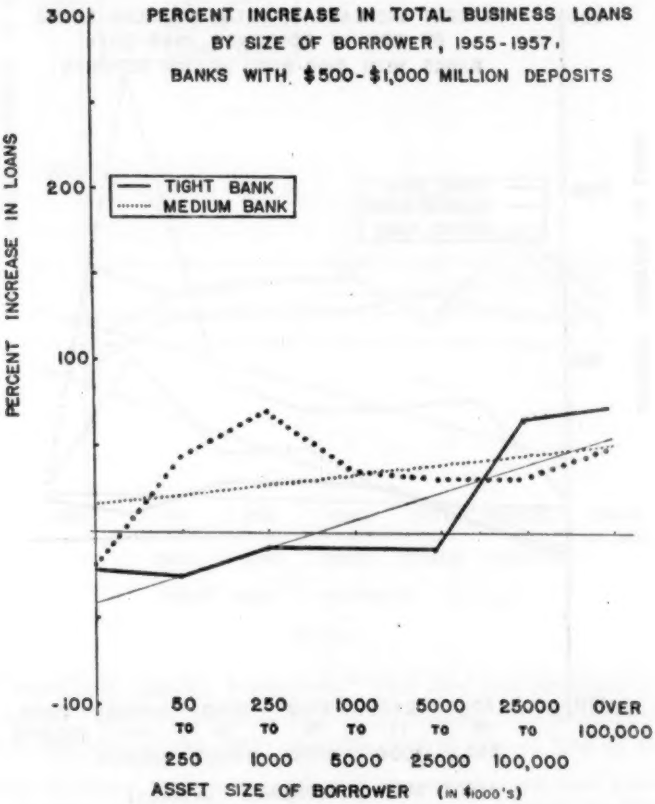

FIGURE 3

in different industries and at banks of different sizes. No clear patterns emerge as between different industries at all banks combined or at banks of different sizes separately. This is not surprising, since there is no a priori reason to expect such size-of-borrower differences as between different industries.[20]

[20] Basic data showing separtely each industry's borrowing from each bank size-group are available for inspection in our files.

In summary, the size-of-borrower data reject the hypothesis that tight money led banks to discriminate substantially against small borrowers in favor of large. Only at banks in the $500-$1,000 million deposit size-group are the data consistent with this hypothesis of substantial discrimination; for the banking system as a whole and for all other size-groups of banks, either the differential behavior at tight and

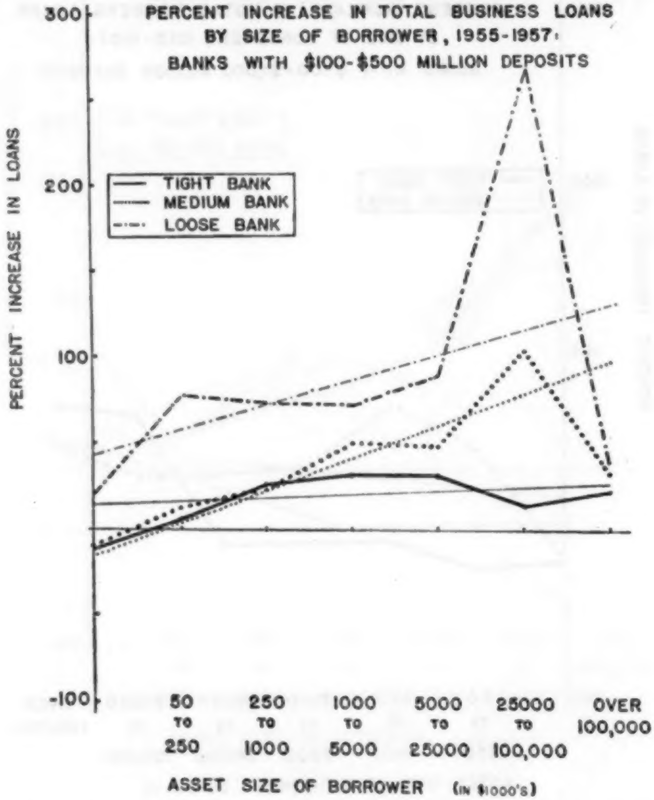

FIGURE 4

loose banks was slight or it was in favor of small borrowers. Crudely, the data suggest that bankers tended under tight money, as would have been expected, to meet their strongest credit-worthy loan demands while in the main adhering to their regular criteria of credit-worthiness; and that in so far as limited discrimination occurred on other bases, bankers may well have tended to care especially for their best customers—at large banks especially larger businesses and at small

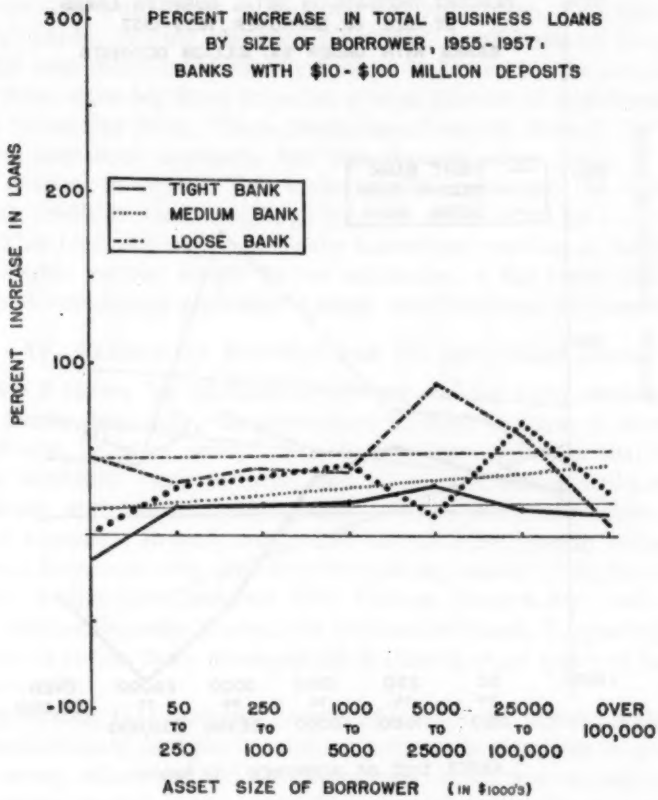

FIGURE 5

banks especially smaller businesses.[21] But this last sentence is based more on the "feel" of the data and on interviews with bankers than on rigorous analysis of the data; and the central fact of lack of substan-

[21] Nearly all bankers, however, deny that they discriminate against small borrowers per se but instead base credit extension on the credit-worthiness and general "goodness" of the applicant, regardless of size. [See 11, pp. 401-2]. Bankers we have interviewed are surprisingly consistent in holding that the most important criterion of a "good" customer is the size of deposit balance he will maintain over the long run, assuming, of course, that he meets the traditional standards of credit-worthiness on individual loans, as most reasonably good customers do.

Some large branch bankers emphasize that lending procedures clearly lead to discrimination *in favor of* small business. Under tight money all large loans must be reviewed by the central loan committee, which is highly sensitive to the scarcity of funds for lending. But branch managers are often left substantially free, under decentralization policies, to make all loans that seem good without central loan committee review as long as the loan is below some prescribed size, for example $25,000.

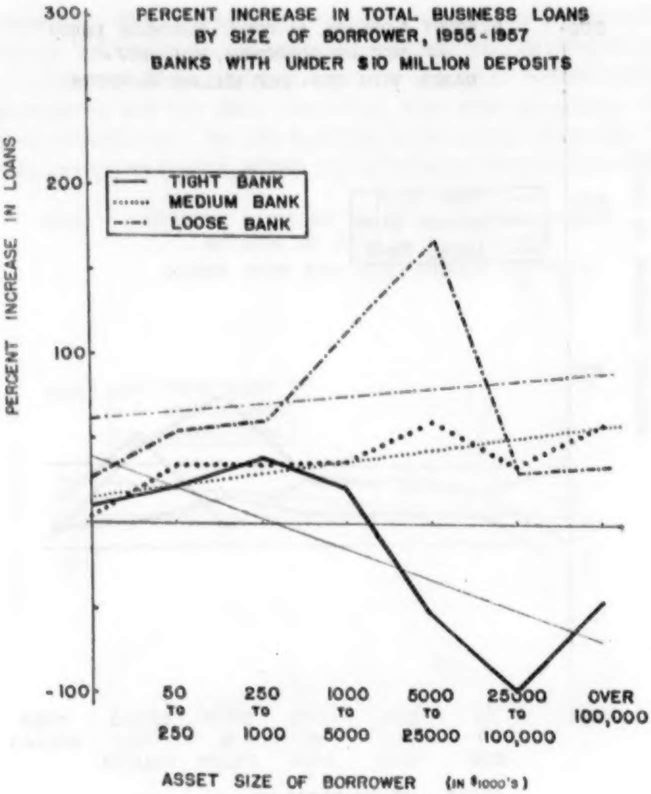

FIGURE 6

tial lender discrimination by size of borrower is the one that emerges from the data.

It is useful to ask directly: Who were the marginal borrowers turned away under tight money—large or small businesses? At loose banks, and at small banks as a class, apparently neither large nor small credit-worthy borrowers were turned away to any substantial extent. Remember the evidence on page 59 that these banks gained more deposits (lendable funds) than they used in extending new credit over the period. In the tight group, large banks and hence large borrowers predominated. Thus, although tight banks probably squeezed both large and small borrowers somewhat, for the banking system as a whole a larger proportion of small than of large borrowers apparently escaped completely the pressure of tight money on their bank borrowing.

A more complete picture of tight money's effects on borrowers of

different sizes would need to take into account lenders other than commercial banks. During the 1955-57 period, small-business borrowing in total rose much more rapidly than is indicated by the commercial bank data, since big firms extended a large amount of additional trade credit to smaller firms. These credits arose mainly through the extension of open-book accounts, but also through other forms of credit from vendors to buyers. Two major studies agree that the rapid rise in trade credit to small from large businesses accounted for a very large sum. This fact, although outside the immediate purview of the present study, adds further weight to the refutation of the claim that tight money discriminated especially against small-business borrowers.[22]

IV. *Business of Borrower and the Investment Boom*

Table 6 shows, for all banks combined and for tight, medium, and loose banks separately, the percentage increase in loans to borrowers in different industry groups. The first column indicates that for all banks combined loans to metal and metal products, petroleum-coal-chemicals, and transportation-public utilities companies showed the largest increases. Indeed, nearly half the total increase in loans to all business borrowers over the two years was accounted for by these three groups. At the other extreme sales finance, construction, real estate, and textiles companies showed the smallest increases. In general, loans to manufacturing firms increased more than to other types of business borrowers.[23]

It is striking that the rapid growth of loans in the metals, petroleum-rubber-chemicals, and public utilities industry groups was in precisely those areas where the 1955-57 investment boom was strongest. In a tight-money period, banks generally increased their loans most to those borrowers who had the strongest loan demands, and in general to those whose business was best and expanding most rapidly. The data thus generally support the proposition that loans were expanded most where loan demand grew most rapidly. For example, within the construction industry loans rose rapidly to large construction firms, whose business rose rapidly during the period, but only slightly to small construction borrowers concerned largely with residential construction, which declined over the period.

Broadly speaking, tight banks under the pinch of tight money used available funds to expand loans where—in manufacturing and public

[22] See especially Allan H. Meltzer [3] and [11, pp. 363 and 482].

[23] The data in Table 6 for all business loans do not agree precisely with those in Table 2 for commercial and industrial loans as to relative increases at tight, medium and loose banks. Part of the difference is due to the inclusion of real estate loans to businesses in the "business loan" figures but not in the "commercial and industrial loan" figures. There may be other factors involved, but if so we do know what they are.

TABLE 6—PER CENT INCREASE IN BUSINESS LOANS, OCTOBER 1955–OCTOBER 1957

Business of Borrower	Per Cent Increase At:				Relative Increase, With Per Cent Increase At Loose Banks = 100		
	All Banks (1)	Loose Banks (2)	Medium Banks (3)	Tight Banks (4)	Loose Banks (5)	Medium Banks (6)	Tight Banks (7)
All borrowers	*52*	*52*	*46*	*56*	*100*	*88*	*108*
All manufacturing and mining:	66	71	56	76	100	79	107
Food, liquor and tobacco	48	8	62	46	100	775	575
Textiles, apparel, etc.	31	1	4	53	100	400	5300
Metal and metal products	98	132	71	118	100	54	89
Petroleum, chemicals, etc.	67	42	49	82	100	117	195
Other manufacturing and mining	59	35	71	53	100	203	151
Trade							
Wholesale	43	65	19	75	100	29	115
Commodity dealers	36	37	12	51	100	32	138
Retail	48	62	45	45	100	73	73
Sales finance companies	27	41	20	28	100	49	68
Public utilities, transportation, etc.	89	23	56	126	100	243	548
Construction	29	33	40	14	100	121	42
Real estate	33	81	41	15	100	51	19
Services	40	56	52	16	100	93	29

utilities—banks as a whole expanded loans most. But the shift of tight banks away from other businesses to these groups was more pronounced than at loose banks. This is shown especially by column 7, which indicates the big relative increases at tight banks in loans to most manufacturing subgroups and to public utilities as compared with loose banks. Conversely, the tight banks showed very small relative increases in loans to construction, real estate, services, and sales finance companies. Again, the evidence is consistent with the proposition that loans rose most where the borrower demand was greatest. The main apparent exceptions are textiles, and food-liquor-tobacco firms, where very large relative increases are shown by column 7 although their aggregate investment growth was moderate. These are both cases where very small percentage increases were reported by loose banks, so even moderate increases at tight banks appear as very large relative increases.

It may be surprising that tight banks increased their commercial and industrial loans more than loose banks over the period, in total, for most of the manufacturing groups, for wholesale trade and commodity dealers, and for public utilities. This was accounted for by the very

large banks—those with deposits over $1 billion—none of which fell in the loose group. At all other tight banks, business loans increased substantially less than at loose banks. Data comparing the lending patterns of tight, medium and loose banks separately for banks in five size classes are presented in Table 7, which is comparable to Table 6 above.

The Table 7 breakdown by size of bank shows substantial diversity; but no pattern of differences in lending behavior at banks of different sizes. This may not be surprising, since there is no a priori reason to suppose that banks of different sizes would react differently in a systematic way to loan demands from different industries. In each of

TABLE 7—PERCENTAGE INCREASE IN BUSINESS LOANS AT DIFFERENT SIZE BANKS, OCTOBER 1955 TO OCTOBER 1957[a]

Size of Bank (Deposits)[b]	Business of Borrower														
		Manufacturing & Mining													
	All Borrowers	All	Food, etc.	Textiles, etc.	Metals	Petroleum etc.	Other	Wholesale trade	Commodity dealers	Retail trade	Sales finance Cos.	Public Utilities, etc.	Construction	Real estate	Services
Over $1 billion:[c]															
% Increase in loans at:															
Medium banks	79	93	119	−25	77	88	210	−13	−2	46	138	131	38	141	90
Tight banks	105	121	81	76	200	118	83	101	74	91	76	193	30	35	37
Relative increase at:															
Medium banks	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100
Tight banks	133	130	68	504	260	134	40	977	3900	198	55	147	79	25	41
$500–$1,000 million:[c]															
% Increase in loans at:															
Medium banks	41	54	51	8	81	36	59	34	93	71	−21	25	92	−6	57
Tight banks	14	12	27	−18	15	−30	50	18	380	11	−25	91	−19	−6	18
Relative increase at:															
Medium banks	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100
Tight banks	34	22	53	−225	19	−83	85	53	409	15	81	364	−21	100	32
$100–$500 million:															
% Increase in loans at:															
Loose banks	75	89	20	−23	312	56	23	126	21	132	−3	−10	65	169	48
Medium banks	39	45	52	17	78	32	27	37	−19	45	16	22	58	39	50
Tight banks	27	27	2	32	39	40	26	65	12	35	20	14	2	8	−2
Relative increase at:															
Loose banks	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100
Medium banks	52	51	260	274	25	57	117	29	−90	34	733	420	89	23	104
Tight banks	29	30	10	339	13	71	113	52	57	27	867	340	3	5	−4
$10–$100 million:															
% Increase in loans at:															
Loose banks	37	24	−2	24	19	32	48	29	52	37	121	48	22	55	54
Medium banks	29	22	8	16	33	21	22	31	19	42	6	39	11	30	38
Tight banks	16	22	17	21	37	28	6	27	−10	40	−24	26	25	11	8
Relative increase at:															
Loose banks	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100
Medium banks	78	92	600	67	174	66	46	107	37	114	5	81	50	55	70
Tight banks	43	92	1050	88	195	88	13	93	−19	108	−20	54	114	20	15
Under $10 million:															
% Increase in loans at:															
Loose banks	47	19	−14	8	10	41	35	54	30	42	41	116	31	63	64
Medium banks	24	11	−8	6	19	55	5	31	54	29	7	49	15	22	27
Tight banks	15	1	24	−43	−17	40	17	21	35	15	−7	108	45	2	11
Relative increase at:															
Loose banks	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100
Medium banks	51	58	143	75	190	134	14	57	180	69	17	42	48	35	42
Tight banks	32	5	371	−537	−170	98	49	39	117	36	+17	93	145	3	17

[a] For corresponding all-bank data, see text Table 5.
[b] Deposits as of October 1955.
[c] No banks in the Over $1 billion and the $500–$1'000 million deposit classes fell in the oose group

the industry groups, small as well as large firms are represented, although in different proportions at different size-groups of banks.

In summary, these data suggest that increasingly tight money during the 1955-57 period was reflected in significantly different increases of loans for different industry groups; and that especially at tight banks, as well as for the banking system as a whole, the loan expansion was greatest to those industries which were expanding most rapidly in terms of plant and equipment expenditure, inventory accumulation, and general level of activity. Thus, broadly speaking, banks increased their loans most where the credit-worthy loan demand was greatest. This does not, of course, say that the rapidly expanding industries necessarily received the most credit *relative to* their loan demands.[24]

But it was not true that bank loans uniformly expanded most rapidly to those industries whose business was growing most rapidly. For example, sales finance companies, whose business expanded rapidly over the period, obtained only a modest increase in bank loans. This was probably in part because they had fairly ready access to the money market through other channels. But it also apparently was because banks generally do not consider sales finance companies highly preferred customers, since finance companies generally do not promise large long-run deposit balances to the extent that many manufacturing and commercial borrowers do.

V. *Interest Rates*

Small businesses generally pay higher interest rates at banks than do large businesses, primarily reflecting differences in size of loan. Small businesses usually borrow small amounts, and investigation charges, servicing charges, and related expenses bulk relatively much larger than on the large loans customarily obtained by large businesses. Large businesses often pay lower interest rates on comparable size loans than do small businesses, but the differences are small and probably reflect mainly differences in risk and in loan-administration costs.

Table 8 shows interest rates paid by borrowers of different sizes in 1955, in 1957, and the net increase over the two-year period. In both 1955 and 1957, the average interest rate paid varied inversely with the size of borrower. But as interest rates rose with tight money over the two-year period, rates to large borrowers were increased considerably more than rates to small borrowers. Over the two years, the spread between average rates to the largest and smallest borrowers declined

[24] The Federal Reserve interview study of bankers in 1957 found "almost complete absence" of any indication of bank policy changes as to the type of industry most desirable to accommodate. Decisions continued to be made on prevailing criteria, though the actual loan distribution shifted with the shifting positions of potential borrowers. See [11, p. 436].

from 2.5 to 2.1 per cent. While the average rate on all new loans rose from 4.2 to 5 per cent, that on loans to large borrowers rose nearly twice as much absolutely, and even more relatively, as that on loans to small borrowers. During the period, moreover, bank requirements that borrowers maintain compensating balances also became more widespread. Since these requirements apply primarily to large borrowers[26] it is probable that differences in effective interest rates narrowed even more than the data in Table 8 indicate.

TABLE 8—INTEREST RATES ON BUSINESS LOANS, BY SIZE OF BORROWER[a]

Asset Size of Borrower (000's omitted)	Average Interest Rate (per cent per annum)		
	1955	1957	Absolute Increase
All borrowers	4.2	5.0	.8
Under $50	5.8	6.5	.7
$50 to $250	5.1	5.7	.6
$250 to $1,000	4.6	5.4	.8
$1,000 to $5,000	4.1	5.1	1.0
$5,000 to $25,000	3.7	4.8	1.1
$25,000 to $100,000	3.4	4.5	1.1
$100,000 and over	3.3	4.4	1.1

[a] Size of borrower as of October 1955. Rates are average rates charged by reporting banks over the July-October period for 1955 and 1957. More detailed data, for loans at different size banks, are presented by the Federal Reserve in [11, pp. 388–89].

This greater increase in rates to large borrowers probably reflected, at least in part, the fact that small borrowers by 1955 were already paying rates near the customary or legal upper limits for nonconsumer loans at many banks. These legal limits are as low as 6 per cent in eleven states, including New York, New Jersey and Pennsylvania, and range up to 15 per cent in others. Thus as interest rates rose, rates to large borrowers could be increased without violating the customary or legal upper limit, while rates to small borrowers could be raised little or not at all. In any case, for the banking system as a whole, it is clear that interest rates to small borrowers rose less than those to large borrowers. In the aggregate tight money did not lead to discrimination in interest costs against small borrowers.

To what extent did tight banks, under the pinch of tight money, use higher interest rates as a device for discouraging especially particular classes of borrowers? Figure 7 shows the change in the distribution of business loans made at different interest rates by tight, medium and loose banks over the 1955-57 period.

The average interest rate charged rose at all three classes of banks.

[26] See [11, p. 433].

Loans made at less than 4 per cent declined at all classes, as the rate structure moved up. The largest percentage increase at both tight and medium banks was in the 4.5-4.9 per cent range, while that for loose banks was in the 5-5.9 per cent range. The apparent differences between tight and loose banks reflect primarily the larger proportion of large banks (and large loans) in the tight group, where rates in the 4.5-5 per cent range represented a large increase for large borrowers. At tight banks, nearly half the total loan volume was in loans of $1 million or more, as compared to less than 5 per cent at loose banks.

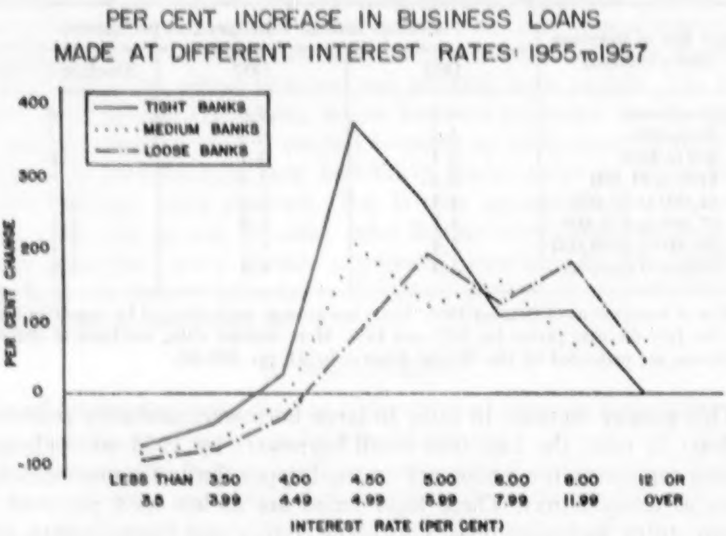

FIGURE 7

By 1957, two-thirds of these larger loans at tight banks were made at 4 or 4.5 per cent, while in 1955 nearly two-thirds were made at rates below 3.5 per cent.[26]

Data showing separately changes in loans made at different interest rates on loans of different sizes at tight, medium and loose banks indicate that tight, medium and loose banks raised interest rates over the period by similar amounts for loans of the same size—though with some wide differences that appear to be random.[27]

In summary, therefore, there is little evidence of much differential

[26] Most borrowers pay about the same rate of interest on their loans, regardless of the size of the bank from which they borrow. See [11, p. 380].

[27] Charts comparable to Figure 7 have been prepared for loans in six different size groups. Copies will be provided on request.

interest rate behavior at tight and loose banks during the period of increasingly tight money. This finding is consistent with the hypothesis that the pattern of interest rates at banks is set by general market forces, and that banks generally follow a policy of price leadership in establishing interest rates, rather than using them as a device to discriminate among borrowers. The hypothesis that tight money raised interest costs especially to small borrowers is clearly rejected by the data.

VI. *Conclusion*

What is the significance of these findings for the use of restrictive monetary policy in the future? Tight money in 1955-57 apparently led those commercial banks which felt its impact to alter their asset portfolios significantly; they shifted to obtain funds to increase loans to profitable borrowers, especially business firms, even at the cost of liquidating government securities on a declining market. Discrimination amongst borrowers was apparently largely on traditional banking standards of credit-worthiness and goodness of borrowers, with differing changes in loans to various borrower groups reflecting primarily differences in loan demands, rather than discrimination by lenders on other grounds, once standards of credit-worthiness were met. Widespread criticisms of tight money as unfairly discriminating against small borrowers, both in availability of loans and interest costs, are not supported by the data.

On the other hand, the fact that increasingly tight banks continued to increase loans to good business customers, whose demand for money reflected partly heavy investment outlays and inventory carrying costs, meant that tight money did not act to deter especially these prime movers in the investment boom. Thus, although tight money in 1955-57 may have led to little "unfair" discrimination against particular borrower groups, it did permit funds to go extensively to the same borrowers who would have obtained them in the absence of tight money. Whether the marginal borrowers shut out by tight money would have contributed significantly to either undesirable investment or inflation cannot be told from these data. Probably at least as much (more, on the objective evidence) of the marginal credit shut off was to large as to small firms, but no comparable generalization as to industry is possible from these data.[28]

Over all, tight money in 1955-57 appears not to have changed greatly the allocation of bank credit among major classes of business borrow-

[28] Unfortunately, the Federal Reserve obtained separate information on loans to new businesses only in the 1957 survey. Thus it was impossible to test the hypothesis that tight money leads banks to discriminate against new businesses. For some evidence on the point, see [11, pp. 390 ff.].

ers from what it would have been with looser money, certainly not by size of firm and only moderately by industry—partly because money was not tight enough to limit seriously loans to credit-worthy customers at a substantial proportion of all banks. Tight money's main effect was apparently to hold down the total volume of credit while inducing credit rationing at tight banks mainly in response to relative strength of demand among "good" bank customers. Whether one evaluates this conclusion as strengthening or weakening the case for restrictive monetary policy may depend largely on his taste for direct controls as against market forces. Tight money helped to restrict total spending and keep the price level down while doing relatively little directly to reallocate resources—the traditional objective of general monetary policy. It apparently did not especially check the industries at the core of the investment boom.

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VARIABILITY IN EARNINGS-PRICE RATIOS OF CORPORATE EQUITIES

By Haskel Benishay*

This study proposes to examine empirically the determinants of the differences in rates of return on corporate equities. The rate of return employed is derived for each equity by dividing the weighted average of annual earnings of nine consecutive years by the market value of the corresponding equity in the ninth year, and will be referred to as the measured rate of return. This empirically derived rate is designed to represent the theoretical ratio of expected income to the market value of the equity, where expected income is the mathematical expectation (mean) of a statistical distribution whose values are earnings expected in future years.

We advance the hypothesis that the measured rate of return of corporate equities is a function of: (1) the trend in earnings; (2) the trend in the market value of the equity (price); (3) the pay-out ratio: the ratio of dividends to earnings; (4) the expected stability of the future income stream; (5) expected stability of the equity value; (6) the size of the firm and the liquidity of its shares, both represented by the market value of the equity; and (7) the debt-equity ratio. Among the independent variables the first three are "corrective": they are expected to remove the errors obstructing a valid measurement of the theoretical concept of a rate of return on equity capital. The remainder are selected to measure the differential "risk" or "desirability" of holding corporate equities, and as such are explanative.

Our method of investigation consists of tracing the relationship between each of the independent variables and the rate of return on equity while holding other independent variables constant in a multiple regression analysis.

I. *The Measured Rate of Return and the Independent Variables*

This section will explain why the selected independent variables may be expected to account for differences in the measured rates of return on corporate equities, and will give the empirical definitions of the

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variables employed in this study. The variables are specified for the firm as a whole, not for a single share.

A. *The Dependent Variable*

The Measured Rate of Return: y. The numerator is a weighted average of earnings after taxes for the cross-section year and the 8 preceding years. This weighted average may be expressed in two ways: the sum of the cross-section year's earnings plus the arithmetic mean earnings of the 9 years, including the cross-section year, divided by 2; or a weighted average such that 10/18 of the weight is given to the cross-section year, and 1/18 of the weight to each of the 8 preceding years.

The equity measure in the denominator of the measured rate is the arithmetic mean of the high and low values of the equity outstanding in the cross-section year.[1]

B. *The "Correctors"*

1. *Growth in Earnings:* X_1. The numerator of the measured rate of return is defined as a weighted average of actual past income, without being adjusted for trend in past income; consequently it may diverge downwards from expected income when past income growth has been high, and upwards when past growth has been low. Inclusion of the trend in past income as an independent variable may allow expected income to be more closely estimated if the market utilizes projections of past income trends for the determination of expected income.

The trend in earnings, X_1, is computed by dividing the coefficient of the simple linear regression of earnings after taxes on time, for the 9 years preceding and including the cross-section year, by the arithmetic mean earnings of the same period (this regression is used later to compute X_4). The division by mean earnings, which is equivalent to a deflation by size, is performed to obtain a measure independent of the dimensions of the firm: a measure of a rate of growth uncorrelated with the size of the firm rather than one of absolute growth.

2. *Growth in Equity Value:* X_2. The incorporation into the model of past trend in the value of the equity may correct for the absence of a recent re-evaluation of expected income in the measured rate of return. The measure of earnings in the numerator of the measured rate of return may fail to reflect an upward change in expected income, while the market value of the equity in the denominator will reflect it immediately. Consequently the measured rate may be smaller than the

[1] The numerator is a crude compromise between assigning equal weights to each of the nine years, and assigning all the weight to the last year. The next obvious improvement would be to find a more appropriate system of weights.

true rate when expected income has risen. A symmetrical argument holds when expected income has declined. The percentage change in equity value, which generally reflects the percentage change in expected income, could be an indication of the extent of the lag with which measured earnings reflect expected income. The larger this percentage change is, per unit of time, the less likely is the empirical representation of earnings to keep up with expected income (the larger will be the difference between true and measured earnings) and the greater will be the negative correlation between growth in equity value and the measured rate of return.

The measure of X_2, trend in equity value, is computed in a manner parallel to the computation of X_1. It is the coefficient of the linear regression (used in computing X_5, stability of equity) of the annual highs and lows of the equity values in 9 consecutive years preceding and including the cross-section year, divided by the arithmetic mean of these same equity values. Here again the division by average equity provides a measure that is comparable cross-sectionally and free of the association between growth and size of firm. This measure denotes past rate of growth in equity or the yearly capital gain per average unit value of equity-holding for the period.

3. *The Pay-out Ratio: X_3, Dividends/Earnings.* A notion seems to prevail in the financial literature, that because investors prefer distribution to retention of earnings, the pay-out ratio and the rate of return are negatively correlated [3, p. 341]. Yet since, on the average, retained earnings are reflected in stock prices and consequently can be realized through a sale, there seems to be no a priori reason for preferring dividend income to capital gains income. Moreover, because of the capital gains tax, the argument may go the other way: retention may be preferred to distribution.[8]

A more reasonable explanation for a negative correlation between the pay-out ratio and the measured rate of return may be provided by examination of the effect of errors in the measurement of earnings.[9] If measured earnings are an overestimate and dividends are a stable proportion of expected income, the rate of return is "too high" and the pay-out ratio "too low." This will introduce into the relationship a neg-

[8] This would be true only if the rate of return on invested retained earnings was not sufficiently lower (in equilibrium) to take away the capital-gains tax advantage, so as to make the after-tax capital gains equal to the dividends (after personal income tax) if earnings had been paid out in dividends.

[9] Two types of error are possible: (a) a difference between the expectation of the population of annual book earnings and expected income, due either to a consistent accounting bias in the measurement of earnings or to the fact that measured earnings, however unbiased *ex post*, may lag behind expected income when the latter has changed abruptly; (2) the difference between earnings measured on the basis of a limited sample (and containing therefore a transitory element) and their population mean.

ative spurious correlation.[4] The introduction of the pay-out ratio as an independent variable in a multiple regression equation is intended to correct for the errors.[5]

The measure employed for X_3 is the arithmetic mean of 3 consecutive annual observations of (dividends paid × 100/earnings), the last observation being in the cross-section year.

C. *The Risk Variables*

1. *Stability of Income:* X_4. (a) For any given level of the firm's capital structure, the larger the variance of the distribution of expected earnings, the larger is the probability of failure. (b) Also, for any level of the capital structure, the larger is this variance the greater is the cost or inconvenience incurred by the investor in maintaining a stable level of expenditure, since borrowing or the carrying of cash balances becomes necessary to counteract income variability. For both reasons, a high stability of income is a desirable property and will tend to produce a low price-earnings ratio.

The measure used for X_4 is a ratio, the numerator of which is the arithmetic mean of earning after taxes, in 9 consecutive years, ending in the cross-section year; its denominator is the standard deviation around the regression of these same 9 observations, on time. If time be *t*, earnings *y*, *m* the sample moment, and *n* the number of observations, then the denominator will be expressed symbolically as:

$$\sqrt{\frac{m_{yy} - (m_{yt}^2/m_{tt})}{n-2}}$$

The whole ratio is a reciprocal of what is essentially a coefficient of variation. The use of mean income in the numerator is designed to produce a relative measure of stability uncorrelated with the size of the firm. If mean income were not put in the numerator, the reciprocal of the standard deviation would be negatively correlated with the size of the firm, since the standard deviation is normally an increasing function of size.

2. *Stability of Equity Value:* X_5. The usual contention is that, since

[4] A similar argument was developed by F. Modigliani and M. H. Miller, in their reply to David Durand [5]. What is described here as a negative correlation due to errors in the measurement of earnings was designated by Modigliani and Miller as a correlation due to the "informational content of dividends."

[5] The above analysis begs the question of whether maximizing stockholder return is really the sole goal of the management of large firms. The management may consistently retain earnings for purposes of self-aggrandizement, not necessarily for profit maximization, in which case retention would be an undesirable characteristic. It also ignores the possibility that insiders may be consistently overoptimistic in regard to new ventures, in which case again retention would be undesirable. The reason for these two omissions is the belief that, although logically admissible, they are realistically improbable.

the precautionary motive for holding a share of stock is dominant, price variability is shunned. When they think about the possibility of being impelled to sell in an emergency, stockholders are presumed to be more averse to a given likelihood of a low price than heartened by an equal likelihood of a high one.

A priori, an opposite hypothesis is also tenable. The speculative rather than the precautionary motive is dominant; therefore equity variability is sought. Stockholders are more encouraged by a likelihood of a high price than discouraged by an equal likelihood of a low one. They prefer stocks with variable prices to stocks with stable ones.

The measure used for X_5 is a ratio. Its numerator is the arithmetic mean of 18 market observations of the firm's equity value: the high and the low for each of 9 consecutive years, ending in the cross-section year. Its denominator is the standard deviation around the linear regression of these same 18 equity values on time. If time be t, equity y, m the sample moment, and n the sample size, this denominator will be written as:

$$\sqrt{\frac{m_{yy} - (m_{yt}^2/m_{tt})}{n - 2}}$$

(where $n = 18$, distributed 2 per year).

3. *Size* (market value of the equity): X_6. This is intended as a measure of both liquidity and size.

(a) Barring radical changes in expectations, larger firms tend to have a higher volume of trading, thereby a more perfect market. Consequently the price at which their shares are sold or bought is less likely to be adversely affected by the transaction of an individual investor. This becomes especially desirable for institutional and other large holders who deal in large blocks.

(b) A larger firm is known about more than in proportion to its size. Therefore the less-informed segments of the market will tend to specialize in holding shares of large corporations. Consequently, what is equivalent to a once and for all shift in demand in favor of larger firms' shares will become a permanent pattern of the market, resulting in these shares' prices being relatively higher.

(c) A larger firm is often considered safer simply because its size represents to many investors better protection against adverse conditions and a smaller probability of failure.

All three arguments suggest that the larger the firm is, the more desirable are its shares. The measure used for X_6 is the arithmetic mean of the high and low values of the equity outstanding in the cross-section year.

4. ***Debt-Equity Ratio:*** X_7. (a) The more heavily a firm's capital

structure is weighted with debt, beyond the optimum, the higher the risk of default. This statement refers to the movement of a single firm along a schedule relating the debt-equity ratio to riskiness. (b) On the other hand, if a firm is at, or approximately at, its optimum debt-equity ratio, the debt-equity ratio is a decreasing function of risk. This relationship clearly relates to the equilibrium pattern that will be attained by a cross-section of firms, such that the lower is a firm's riskiness the higher is its optimum debt-equity ratio. Thus the debt-equity ratio may represent either risk or safety, depending on the context in which it is used. Consequently it becomes important to ascertain whether the debt-equity ratio employed in this study in effect reflects deviations from its optimal position in each firm, or instead is a measure of these optimal points themselves.

If by holding size and income stability constant, as will be done in the regressions, we consequently hold fixed the main determinants of the debt-equity ratio, namely variables to which the debt-equity ratio is adjusted by management in an attempt to maintain optimal capitalization, then X_7 will come to represent deviations from equilibrium. In such a case we should expect a positive sign for the debt-equity coefficient (i.e., the higher the debt, the larger the risk of default, the less valuable the equity and the larger the rate of return). If the debt-equity ratio is mainly a function of other risk variables not included in the regression (or if it depends on the level of profit expectations, an increase in which may motivate a debt-financed expansion for the purpose of leverage gains), then the debt-equity ratio will reflect a desirable characteristic, and its coefficient will have a negative sign. The measure used for X_7 is the book value of debt at the end of the cross-sectional year divided by X_6.

II. *Empirical Results*

This study consists of a comparison of 56 companies in the four years, 1954, 1955, 1956, and 1957, with each firm constituting an observation in a cross-sectional multiple regression analysis.

The principal sources of data were *Moody's Industrial Manual* [9] and *Moody's Handbook of Widely Held Common Stocks* [8]. Industrials with comprehensive and comparable income statements for nine consecutive years preceding and including the cross-section year were chosen. Use was also made of the *Bank and Quotation Record* and the *Commercial and Financial Chronicle*. *Moody's Handbook of Widely Held Common Stocks* however, provided most of the raw data.

The firms were chosen with the additional criterion of having common but no preferred stocks.[6] This step was necessary because of ob-

[6] The firms in the four cross-sections were all the industrial firms in *Moody's Handbook*

stacles involved in an unambiguous computation of the stability of equity value, growth in equity value, and the pay-out ratio, when both common and preferred equities are outstanding. In the case of these three variables there is no composite measure for which it is possible to assign unequivocally an appropriate weight to the separate measurement of the variable for each of the two types of equity. To illustrate: the pay-out ratios for the two equities are usually different, whereupon a problem of weighting arises in deciding how to combine them into a single measure.

Our model was tested by running cross-sectional linear regressions, in which logarithmic values were used for y, X_3, X_4, X_5, and X_6, and actual values for the other variables. The variables X_1, X_2, X_4, and X_5 were computed for the 1954 and 1956 cross-sections; their 1954 computation was used in both 1954 and 1955 regressions, and their 1956 computation was applied to both 1956 and 1957 regressions.[7]

The results are now matched against the theoretical contentions. For convenience, we shall refer to a partial regression coefficient simply as a coefficient and to the changes in the t-ratios of these partial regression coefficients simply as changes in the coefficients. We start by noting the comparative regression performance of two correctors, growth in earnings and growth in equity, and then choose for further use one of the two which a study of the regressions reveals to be the more successful corrector. Our criterion of success is mainly the extent of the negative relation between the growth measure and the measured rate of return: the stronger is this relation, the more successful is the growth measure. We make this choice because we believe that entering both growth variables in the same regression would be illegitimate.

Certainly the inclusion of both growth variables will enable us to observe the relationship of each with the measured rate of return while the other is held constant (which is a consequence of multiple regression), but will also introduce a positive spurious correlation between growth in earnings and rate of return as well as a negative one between growth in equity value and rate of return. *Ceteris paribus,* when growth in equity value is held constant, a higher growth in measured earnings is necessarily associated with a higher measured rate of return. Thus, the inclusion of both growth variables may enable this tautological and positive relation to counteract or even reverse the expected negative regression relation between the measured rate of return and growth in earnings. Also, *ceteris paribus,* when growth in

of Widely Held Common Stocks that fit our requirements and had complete and unambiguous data either in the *Handbook* or in *Moody's Industrial Manual.*

[7] Nine more regressions were run for each year to investigate the effect of dropping, adding, and substituting variables [1, pp. 65-67].

measured earnings is held constant, a higher growth in equity value must be related to a low measured rate of return. Thus the inclusion of both growth variables may enable this obvious negative association to superfluously increase the expected negative regression relation between growth in equity value and the rate of return.

In Table 1 we present three regressions in order to compare the performance of the two growth variables and choose the better corrector. The first, (1), contains measures for all the independent variables of the model, including the two growth measures. The second, (2), includes measures for all but growth in earnings. The third, (3), includes measures for all but equity growth.

Growth in Earnings: (X_1). Whether X_1 will emerge in the regressions with a significant negative sign depends on the extent of the positive correlation between growth in earnings in two adjacent periods (or more specifically upon the extent that the market extrapolates past growth trends in earnings). If this correlation be high then a measure of past growth in earnings may be a good predictor of future growth in earnings and as such may be related negatively to the measured rate of return.

The growth-in-earnings measure, X_1, has a nonsignificant negative coefficient in regression (3) where X_2, growth in equity value, is absent. This seems to indicate that the trend of the series of past earnings has some but not much importance in correcting the measured rate of return. Indirectly, it suggests a relatively low positive correlation between growth in earnings in two consecutive time intervals.

In regression (3), X_1 is the only measure of growth entered; in (1), X_2 equity growth is also included. Since X_2 is positively correlated with X_1, and is better correlated with y, X_2 "robs" the corrective function of X_1 in regression (1) and even causes X_1 to reverse signs in two of the four years. In fact when both growth variables are entered, the growth-in-earnings coefficients tend to become more positive and those of growth in equity value more negative as was expected.

Growth in Equity Value: (X_2). We have argued previously that the larger the recent change in equity value, concomitant with a change in expected income, the larger the lag behind expected income of the earnings measure used in the rate of return. By introducing rate of growth in equity value as an independent variable, and utilizing its negative correlation with the rate of return we expect to correct for this error in the measurement of expected income.

In regression (2), where growth in equity value X_2 is the only growth variable, the coefficients of X_2 generally emerge with a significant negative sign. This result clearly points in the theoretically anticipated direction. It indicates that growth in equity value serves as a

TABLE 1—PARTIAL REGRESSION COEFFICIENTS AND THEIR STANDARD ERRORS (IN PARENTHESES)

Regression	X_1 Earnings Growth	X_2 Equity Growth	Log X_3 Pay-out Ratio	Log X_4 Earning Stability	Log X_5 Equity Stability	Log X_6 Size	X_7 $\frac{\text{Debt}}{\text{Equity}}$	R^2
				1954				
(1)	−.0002 (.0037)	−.0052 (.0030)	−.543 (.175)	−.104 (.067)	+.171 (.116)	−.111 (.031)	−.00010 (.00088)	.507
(2)		−.0052 (.0024)	−.538 (.152)	−.104 (.067)	+.173 (.113)	−.111 (.030)	−.00009 (.00085)	.507
(3)	−.0037 (.0031)		−.529 (.178)	−.112 (.069)	+.176 (.118)	−.126 (.030)	+.00004 (.00090)	.477
				1955				
(1)	−.0004 (.0004)	−.0039 (.0030)	−.242 (.173)	−.099 (.068)	+.102 (.121)	−.090 (.030)	−.00077 (.00090)	.369
(2)		−.0041 (.0025)	−.235 (.158)	−.100 (.067)	+.105 (.116)	−.091 (.030)	−.00074 (.00085)	.369
(3)	−.0003 (.0031)		−.225 (.173)	−.107 (.068)	+.101 (.121)	−.101 (.029)	−.00071 (.00090)	.347
				1956				
(1)	+.0001 (.0024)	−.0067 (.0037)	−.342 (.187)	−.141 (.085)	+.160 (.132)	−.084 (.031)	−.00179 (.00110)	.423
(2)		−.0066 (.0033)	−.345 (.180)	−.143 (.082)	+.161 (.130)	−.084 (.031)	−.00181 (.00107)	.423
(3)	−.0015 (.0021)		−.147 (.164)	−.185 (.080)	+.295 (.109)	−.080 (.030)	−.00181 (.00077)	.423
				1957				
(1)	+.0014 (.0026)	−.0076 (.0038)	−.380 (.170)	−.079 (.090)	+.131 (.146)	−.079 (.035)	−.00195 (.00086)	.375
(2)		−.0067 (.0033)	−.403 (.164)	−.089 (.088)	+.133 (.145)	−.079 (.033)	−.00208 (.00083)	.371
(3)	−.0007 (.0024)		−.260 (.164)	−.107 (.092)	+.311 (.117)	−.102 (.034)	−.00182 (.00089)	.325

good corrector for a deficiently measured rate of return and lends support to the contention that book earnings, however correct *ex post*, tend to lag behind expected income. This result establishes one interesting aspect of the unavoidable error in the measurement of expected

income, and corrects the measured rate of return in the right direction.

Which measure of growth shall be used? We have learned from the empirical results that the trend in equity value, X_2, performs better than the earning trend, X_1, as a corrector for a deficiently measured rate of return. When X_2 is substituted for X_1 in regressions (2) and (3), the total coefficient of determination increases and coefficients of the other independent variables become more stable over time. Moreover as X_1, growth in earnings, is added to the regressions already containing X_2 [regressions (2) and (1)], the addition to the total explanation is barely noticeable; and the effect on the performance of the other independent variables is minute. When X_2, growth in equity value, is added to the regression where X_1, growth in earnings, has already been included then the addition to the explanation of the variance of y is appreciable; and the performance of the other independent variables is affected, including, of course, the reduction in expected performance of X_1 to less than statistical significance. On the basis of a superior performance of X_2 and the fact that the coefficients of the other variables are more stable over time with X_2 rather than X_1 in the regression, we shall choose regression (2) for subsequent exposition of remaining empirical results.

The Pay-out Ratio (X_3). The dominant feature characterizing the performance of the pay-out ratio is a negative statistically significant coefficient, indicating that the higher is the pay-out ratio the higher is the value of the firm.

We have already rejected as an interpretation of this result that, *ceteris paribus,* investors prefer distribution to retention of earnings. Instead, the pay-out ratio may represent, in the capacity of an instrumental variable, the extent of error in the measurement of expected income. The more measured earnings overestimate expected income, the higher is the measured rate of return and simultaneously the lower the measured pay-out ratio. Consequently it is to be expected that the pay-out ratio would be negatively associated with y, and would thus play a useful part in the regressions. We derive additional support for this view from the fact that the pay-out ratio coefficients become substantially more significant in the negative direction with the inclusion of growth variables in the regressions [1, pp. 65-67]. This is best exemplified in the 1956 multiple regression of log y on X_1, X_2, log X_3, log X_4, log X_5, log X_6, and X_7. The partial regression coefficient of log X_3 and its standard error were respectively .342, .187. In the same regression, except for the omission of X_2, the partial regression coefficient of log X_3 and its standard error were respectively .147, .164.

The relation of X_3 with the growth measures suggests that the particularly successful performance of X_3 may be due to the inclusion of

growth measures in the regressions. The relationship of X_3 with the rate of return appears in the regressions when growth is held constant. For the same growth rate, a company with a higher pay-out ratio is a company which was more successful in the past and may be expected to continue to be more profitable in the future. Alternatively stated, growth is in fact higher when it is not paid for by low dividends. The firm with a higher pay-out ratio perhaps grew to a larger extent because of improvement in cost conditions and/or favorable shifts in demand and to a lesser extent because of retained earnings. Hence, when growth is included, X_3 indicates a relatively higher level of expected income. In this role X_3, the pay-out ratio, serves as a corrector for an incomplete measurement of expected income in the measured rate of return.

To summarize: The fact that X_3 coefficients become more significantly negative when growth measures are included in the regression lends support to the argument that the negative association between X_3 and y is due to errors in the measurement of expected income, and reinforces the claim that X_3, the pay-out ratio, is a corrector for the deficiently measured expected income in the measured rate of return.

Stability in Earnings (X_4). The partial regression coefficient of X_4 in the four cross-sections is consistently negative, although only barely significant. Thus, the hypothesis of a preference for earnings stability is modestly supported.

It is also noteworthy that X_4 performs better in the direction predicted when X_5, stability in equity value, is present in the same regression. In the absence of X_5 the t-ratios of X_4 coefficients drop considerably [1, pp. 65-67]. The reinforcement of X_4 by the presence of X_5 is substantial. In the absence of X_5, the positive relation between X_5 and the rate of return, y, rubs off on X_4 due to a positive correlation between X_4 and X_5 (the earning stability and the stability of equity value).

Stability of Equity Value (X_5). Do investors find the stability of equity value desirable or undesirable? The coefficient for stability of equity value appears with a positive sign, and modest t-ratios. Its reliability is fortified by consistency rather than by the level of significance in the separate cross-sections. It indicates that the market has an aversion to, rather than a preference for, the stability of equity values thus lending support to the less orthodox of the two alternative points of view presented in this connection.[8]

The positive coefficient for X_5, stability of equity value, is enhanced

[8] Other hypotheses may also be consistent with the performance of stability of equity value, but the "aversion to equity stability" was the one chosen in advance since it seemed the most reasonable.

when X_4, stability of income, is included in the regression [1, pp. 65-67]. In two of the four cross-sections the sign is reversed from negative to positive, and in others the X_5 coefficient becomes more significantly positive. The relation of X_5 to X_4 seems to be parallel to the relation of X_4 to X_5. The negative coefficient of X_4 is enhanced when X_5 is included, and the positive coefficient of X_5 is enhanced when X_4 is included. The regressions show the relationship of each with the rate of return, while the other is held constant.

Size (X_6). We claimed that shares of stock of larger firms are more easily sold without significantly affecting market price, that larger firms are better known and therefore their shares of stock are more in demand, and that larger firms may be considered safer simply due to their size. Consequently we predicted a negative relation between size and the measured rate of return. Our results strongly support our expectation. The performance of the size coefficient compares favorably with that of all other variables in the model. Its performance over time is the most consistent. The signs of the coefficients are negative throughout, and their t-ratio is always well over 2. Thus the contention that larger firms are preferred in the market is well supported.[9]

The Debt-Equity Ratio (X_7). The debt-equity ratio results are difficult to interpret. In all four cross-sections the coefficient for X_7 is negative [regression (2)], and in 1956 and 1957 it is quite significant. This indicates that the higher is the debt-equity ratio the lower is the measured rate of return, or a higher value for the equity is associated with a higher debt.

In discussing the rationale for including X_7 among our independent variables, we pointed out that the debt-equity ratio can be interpreted in two ways: (1) If it represents deviations from its equilibrium value, then it ought to be positively associated with the rate of return. (2) If it were to be measured at its equilibrium level it ought to be negatively correlated with the rate of return, since then it becomes a measure of safety. The results, prima facie, support the latter interpretation.

But in the light of the particular variables entered in the multiple regression, still another interpretation is admissible. If a relevant measure of size is the combined value of both equity and debt, then for a given value of equity the debt-equity ratio becomes a complementary measure of size. The higher the debt-equity ratio for a given equity, the larger is the sum of equity and debt. In the multiple regression, X_6,

[9] In this connection, if X_6 is measured with an error (measured equity value being randomly distributed around "true" equity value) a negative bias may be built into the regression relationship between the log of the measured rate of return and log X_6. This may be true since X_6 constitutes the denominator of the measured rate (y/X_6), and an error in X_6 will affect both sides of the regression equation in opposite directions. However, in this study such errors have only a minor effect [1, pp. 59-69].

equity value, is entered along with the debt-equity ratio as an independent variable. Therefore, the regression relationship of the debt-equity ratio with the measured rate of return is determined while holding equity value constant, which in turn may make the debt-equity ratio in the context of this study a measure of size and thus account for its negative relationship with the dependent variable.

III. *Conclusions*

The strongest result is in the case of X_6, the size variable. Its performance constitutes a handsome realization of expectations: it is consistent and the most significant statistically. It indicates a negative relation with the rate of return in all four cross-sections firmly establishing that, *ceteris paribus,* the market prefers larger to smaller firms.

The most interesting result is the emergence of the coefficients for stability of equity value with positive signs in all four cross-sections. Contrary to the commonly accepted notions that equity stability is preferred, the results lend support to the theory that equity stability is avoided. This result merits further theoretical as well as empirical attention.

The performance of the coefficients for stability of income is as expected; the coefficients emerge with negative signs indicating a preference for stability of earnings.

The debt-equity ratio relationship shows inconclusive results difficult to interpret unequivocally. Although in all four cross-sections its coefficients are negative, it is unwarranted to conclude that a high debt-equity ratio is an indicator of a desirable characteristic, since in the context of this study the debt-equity ratio could be mainly a measure of size thereby obliterating its use as a measure of risk. One possible improvement would be to enter in the regression the sum of equity and debt, thereby insuring that the debt-equity ratio does not serve in fact as a measure of size. Another obvious empirical improvement over the method used in this study would be to define equity in the denominator of the debt-equity ratio as an average of a few years preceding the cross-section year itself. This might rid the debt-equity ratio of a random component, which is built into the empirical definition by using only the cross-section-year average for equity.

The function of the growth variables was visualized as the correction of the measure of expected income in the numerator of the measured rate of return. In this capacity their coefficients were expected to have negative signs. The growth variables performed as was expected.

In its capacity as a corrector the pay-out ratio produces significantly negative coefficients. These are likely to stem from errors in measurement of income due either to the transitory nature of any period's

actual earnings, which renders these earnings different from their mean, or to accounting misrepresentation of both actual earnings and expected income.

Finally, it is hoped that this study will stimulate awareness of difficulties involved in the measurement of both the dependent and independent variables and that the distinction between corrective and explanatory variables may be employed advantageously in further work.

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FINANCIAL INTERMEDIARIES AND THE LOGICAL STRUCTURE OF MONETARY THEORY

A Review Article

By DON PATINKIN*

Specialization is the essence of economic life. And the particular aspect of specialization which John G. Gurley and Edward S. Shaw have undertaken to analyze in their recent—and long-awaited—book[1] is that between earning income and disposing of it. It is this specialization that is "the basis for debt, financial assets, and financial institutions" (p. 17). Conversely, the existence of such financial arrangements is a necessary condition for the transfer of funds from savers to investors. Thus the ability of an economy to draw resources to their most efficient uses depends in a crucial way on the efficiency of its financial system (p. 56).

The workings of this financial system are studied by Gurley and Shaw (henceforth referred to as G-S) in a fresh and provocative way. Indeed, this is their major contribution: the detailed presentation of a conceptual framework from which they fruitfully reconsider old and familiar problems, and fruitfully undertake the analysis of new and unfamiliar ones.

I. *The Main Argument*

The main theme of the book is the development of "a theory of finance that encompasses the theory of money, and a theory of financial institutions that includes banking theory" (p. 1). By this first objective is meant the presentation of the theory of money as part of a general theory of optimum portfolio selection (p. 57). Similarly, by the second objective is meant the presentation of the theory of the banking system as part of a general theory of the choice of optimum portfolios of assets and debts by financial institutions of various kinds.

In accomplishing their first objective, the authors follow in the footsteps of Keynes, Joan Robinson, Hicks, and Tobin—to whom they make explicit acknowledgments (p. x). In this connection they provide a wealth of instructive and illuminating detail on the overriding objective of risk-avoidance which leads individuals to diversify their portfolios (p. 117), and on the comparative advantages and disadvantages of the various assets amongst which this diversification is carried out [tangible assets, short- and long-term

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[1] *Money in a Theory of Finance*, by John G. Gurley and Edward S. Shaw, with a Mathematical Appendix by Alain C. Enthoven. Washington, D.C.: The Brookings Institution, 1960. Pp. xiv, 371. $5.00.

bonds, constant-purchasing-power bonds, stocks, and "blue chips" (pp. 32-33, 159-73)]. G-S then go on to show that there are forces leading to security differentiation on the supply side as well. In brief, "excess demands, positive or negative, for current output are of necessity excess supplies of securities, and the sectoral location of excess demands partly determines the types of primary securities that will be issued. . . . Sharecroppers cannot issue commercial paper, or farmers corporate bonds, or business firms Treasury bills. . . . The real world and the financial world are one world" (pp. 120, 122-23).

At the same time, G-S explicitly disclaim any intention to "advance in the least the theory of risk and uncertainty" (p. 10; also p. 92). Furthermore, their statement of the conditions that define an optimum portfolio is, to say the least, vague. This optimum is merely described (for a portfolio consisting of tangible assets, bonds, and money) as a situation which obtains when there exists a certain imprecisely specified relationship among the "marginal rental rate" (i.e., marginal productivity of capital—p. 26), the rate of interest, and the "implicit deposit rate" (pp. 32-33; also bottom of p. 119, and p. 127). By this last is meant some measure of the traditional transactions, precautionary, and speculative benefits of holding money, in addition to the possible benefits to be derived from a decrease in the price level (pp. 31-33, 70-71, 151-53). But nowhere is this rate explicitly defined. Indeed, there is not even an unambiguous statement of its dimensions! At one point (p. 32, top) it seems to have the dimensions of utility, while at another (p. 152) it seems to have those of a percentage, or even of money.[8]

Thus it is not in the direction of the pure theory of portfolio selection that G-S's contribution lies. This, instead, is to be found in the accomplishment of their second objective: namely, the analysis of "financial intermediaries," that is, "financial institutions whose principal function is the purchase of primary securities and [by?] the creation of claims on themselves" (p. 363). Here G-S are breaking new ground all the way. First, they bring "nonbanking" or "nonmonetary" financial intermediaries [e.g., insurance companies, savings and loan associations, mutual savings banks, pension funds (p. 193)] out of the limbo to which they have been relegated by most economic theory, and analyze them both from the viewpoint of their influence on the "real" variables of the system (namely, the rate of interest, and thereby the levels of savings and investment), and from the viewpoint of their impact on the banking system.

Second, they most stimulatingly show (and this is one of their main themes) that the banking system itself is only one (albeit, usually the most important one) amongst many different kinds of financial intermediaries. In one of those striking passages that recur throughout the book they write:

> There are many similarities between the monetary system and nonmonetary intermediaries, and the similarities are more important than the

[8] This dimensional confusion is particularly noticeable on page 66 where we are told that an optimum position obtains when there exists "a balance among marginal utility of consumption, the rate of interest, and the marginal implicit deposit rate for real money." This passage also illustrates the imprecision of G-S's description of the optimum—for what is the meaning of "balance"?

differences. Both types of financial institution create financial claims; and both may engage in multiple creation of their particular liabilities in relation to any one class of asset that they hold (p. 202).

> The difference between the monetary system and nonmonetary intermediaries in this respect, then, is not that one creates and the other does not, but rather that each creates its own unique form of debt. . . . Money is unlike other financial assets, for it is the means of payment. Corporate stocks are unlike other financial assets, too, for they carry ownership rights in corporations. And policyholders' equities in insurance companies are different because they are linked to certain insurance attributes (pp. 198-99).

Similarly, G-S insist on analyzing the economic behavior of the banking system—like that of any other intermediary—in terms of its attempts to achieve an optimum portfolio of assets and debts within the restrictions imposed upon it. This approach is to be contrasted with the usual tendency of economists to treat the banking system as the Cinderella of monetary policy: as a parasitic member of the community, permitted to earn its living by doing something which—if it only wanted to—the government could do without cost (namely, creating money); as a member whose existence is accordingly suffered, and on whom we can therefore impose without compunction any restrictions deemed necessary by monetary policy. What G-S have shown is that the banking system has a purpose and a soul, and that if its welfare is not properly considered, it will wither up and die. The acceptance of G-S's approach should go far toward eliminating that War of Amalek between economists and bankers with which we feel it our duty to indoctrinate our beginning students.

One of the main questions that concern G-S is the relationship between the real and the financial in the growth process. On the one hand, they emphasize that improvements and innovations in "financial technology" can speed up this process by expediting the flow of funds from savers to investors, and thereby reducing the rate of interest. Under the heading of such technological improvements come improved distributive techniques (better communications, lower transactions costs, and generally the perfection of a well-developed securities market) and the emergence of financial intermediaries, primarily of the nonmonetary type (pp. 123-26).

On the other hand, G-S continuously stress that an integral part of the growth process is an increasing demand for real financial assets—in the form of both money and securities. Correspondingly, one of the questions to which they repeatedly return is the "neutrality" of money in this process: whether it makes any difference to the economy whether the increased real money balances it demands are supplied by an increase in the nominal quantity of money or by a decline in the absolute price level. Here the authors reconfirm the conclusion that, in the absence of money illusion, distribution effects, and rigidities, it does not make any difference. At the same time, on the familiar grounds that these assumptions are not likely to prevail in the real world, they make clear their own preference for accompanying the growth process with a concomitant growth in the nominal money supply (pp. 179-87). They also nicely emphasize that by adopting a policy of increasing nominal money

at a constant price level, the government is in effect borrowing from the private sector at a zero interest rate, and can make use of these borrowings to finance an investment program that will further speed up growth (pp. 41-42). Alternatively, it can accomplish the same result by injecting the new money into the economy in such a way as to lower the rate of interest (pp. 185-87).

G-S also provide a formal analysis of the implications of the balanced-growth process for the stock of securities in the economy (pp. 95-112). There are, however, some loose ends to this analysis.[3] But my main reaction is to attach considerably less importance to this analysis than the authors seem to: for the balanced-growth assumption precludes from the outset just those possibilities of changing portfolio compositions and changing money velocities that should be of primary interest to monetary theorists (cf., e.g., pp. 110-11).

A recurrent and basic theme of G-S's book is that the growth in the totality of financial assets has a "scale effect" on the demand for any one given financial asset, and for money in particular (pp. 167, 170, 174, 177-78, 203). It is here that I find some of the most questionable aspects of G-S's argument. First, this totality of assets is measured in a gross way, without netting out any concomitant increase in debt. Second, the influence of these assets is conceived as something distinct from the influence of real wealth (p. 173, bottom). All this is part of G-S's repeatedly emphasized "gross money doctrine," which will be discussed in detail in the next section of this review. There it will be shown that G-S's "doctrine" stems from a basic misunderstanding. It will also be argued that the proper variable for the analysis of demand for any good, including money and securities, is total *net* wealth, including wealth in the form of financial assets.[4]

A great virtue of G-S's book is that the authors tie in their theoretical analysis with policy recommendations. Furthermore, it is abundantly clear to

[3] I have in mind the following points: (a) In their analysis of the debt-income ratio in equation (6) on page 105, G-S do not even advert to the possibility of n (the rate of growth) being less than i (the rate of interest). Nor do they consider the case in which the marginal productivity of capital equals the rate of interest, so that the debt-income ratio reduces to the capital-output ratio. (b) It is contended that the existence of cyclical movements "is a drag on the growth of primary securities and financial assets" (p. 115). But it is not explained why this should cause the growth to be smaller over the period as a whole than that which would have taken place if the economy had expanded smoothly at the average rate of growth which obtained. (c) Ambiguity also surrounds the discussion of "mixed asset-debt positions," e.g., the position of a firm which issues securities in order to finance the holding of money (pp. 112-13). This will clearly increase the *absolute* supply of securities. But it should have been made clear that as long as the degree of "mixedness" remains constant there is no effect on the *rate* of growth of outstanding securities.

[4] I hasten to add that my own treatment of this question in *Money, Interest, and Prices*, Evanston, Ill., 1956, is not satisfactory: for there too demand is *not* represented as being dependent on the aggregate value of physical and monetary assets together (p. 126; see also pp. 205-6). On the other hand, it is not, strictly speaking, always correct to assume that the dependence of demand is on total wealth—that is, solely on the discounted value of the income stream. Under certain assumptions, it can be shown that the time-shape of the income stream also affects demands.

the reader that the authors have built their theoretical structure on the basis of a close and intimate knowledge of the realities of a modern financial system. Indeed we are explicitly told (p. vii) that the book is the outgrowth of an empirical study of financial institutions in the United States, the complete version of which is yet to be published.

One of G-S's policy conclusions is that there should be a greater integration—if not consolidation—of monetary and government-debt management. "Government debt, on the one hand, and debts of the monetary system on the other, are each so important a segment of total financial assets that management of them by different authorities working for dissimilar goals must be expensive in real interest costs to the Treasury, in monetary stability, or in real earnings of the banking system" (p. 280). This is not an unfamiliar contention, but it receives new force when viewed from the conceptual framework which G-S provide.

More novel conclusions are that monetary policy does not require either a legal or customary minimum reserve ratio; that, indeed, such a ratio forces the private banking sector frequently to become a "disequilibrium system"—for it is forcefully prevented in this way from achieving its optimum portfolio; and that, accordingly, a policy of paying banks a variable rate of interest on their deposits at the central bank (including the possibility of imposing negative rates) is a more desirable, and more flexible, means of influencing the banking system so as to contract and expand in accordance with the desiderata of monetary policy (pp. 266-71, 289).

Finally, and most interestingly, G-S argue that the choice of the banking system as the sector through which monetary policy is usually effected has worked to the disadvantage of this system as compared with other types of financial intermediaries. It is to this that they attribute the declining net profit-asset and capital-asset ratios of the banking system. Their suggested solutions for this state of affairs range from 100 per cent money and a nationalized bank system to the spreading of controls over nonbanking financial intermediaries as well, "so that the burden of control is distributed more evenly among issuers of financial assets." These controls "may also be extended to . . . issuers of primary securities, as illustrated by consumers credit regulations or by private capital issues restrictions" (pp. 288-91).

Whether or not one accepts these conclusions, there can be no doubt that future discussion of these problems will have to consider the points raised by G-S, and can also profitably make use of the analytical framework they have provided for this purpose. In brief, their book is one that must be read by every serious student of monetary theory and policy.

At the same time I must note that G-S's book suffers from some serious failings. First of all, its language is occasionally woolly and imprecise, and too frequently tends to the overdramatic (cf., e.g., pp. 39-40, 118, and especially 141-42). More important, the book is much too repetitious—to a degree that goes far beyond the degree of "judicious repetition" dictated by the author's perfectly justified plan of proceeding from simple to more complicated models. What is accomplished by describing the nature of the

demand for money in essentially similar ways first on pages 32-33, then on pages 70-71, and finally on pages 150-53? And the same may be asked for the triple discussion or the neutrality of money on pages 39-46, 82-88, and 179-87. Why is security diversification discussed under three separate headings on pages 70-72, 117-19, and 150-52, respectively? This repetition is not only tiring but at times actually confusing. There is no doubt that *Money in a Theory of Finance* would have been a better book, and one with a much sharper impact, if it had been cut considerably.

On the other hand, despite the repetition, the reader is sometimes left in the dark as to the nature of the assumptions on which the argument at specific points is based. Thus he must infer for himself that the discussion of Chapter 4 is based on the assumption that all money is of the "inside" variety (see next section), and the same is true for Chapter 7. Similarly, only later does it become clear that the argument on pages 79-81 assumes a balanced-growth process. And he is left to extend for himself the flow-of-funds table of page 22 to the discussion on pages 58-59, where it is really essential for a full understanding of the argument.

The second failing of the book stems from G-S's straining at "iconoclasm" (p. ix). This is what leads them to some of the stylistic excesses already noted. More important, it prevents them from lending a sufficiently understanding ear to some of the teachings of the Old Gods, including the Keynesian ones. As a result G-S sometimes misinterpret these teachings. And sometimes they fail to see the deeper relationship between these teachings and their own, a fact which prevents the reader (and frequently the authors too!) from seeing the full significance of the argument. We will see the main examples of this in Sections II and III below.

The last failing of the book is that it is involved in serious error at some points which are fundamental to its theoretical structure. This is what will be shown in Sections II and IV below.

Not much will be said here of the Mathematical Appendix to the book, which was written by Alain C. Enthoven. This gives a good deal of emphasis to the problem of balanced growth. I might, however, point out that this appendix makes repeated use of Samuelson's "Correspondence Principle" without really answering the criticisms which have demonstrated its limited applicability (p. 328, n. 25). My main criticism, however, is on the grounds of omission: There is not always full communication between appendix and text. More important, the appendix does not treat just those crucial and incorrectly analyzed issues referred to in the preceding paragraph; and what makes this omission particularly unfortunate is that the errors are of a type that might have been caught by a mathematical cross-checking of the argument. All this will become clear from what follows.

II. *"Inside" and "Outside" Money*

One of G-S's significant contributions is in bringing to the fore the distinction between money based on private domestic debt ("inside money") and

money of a fiat nature or based on any other asset ("outside money"). Nevertheless, as already indicated, the analytical developments which G-S base on this distinction are themselves involved in error. In order to show this, we must first describe G-S's model in somewhat greater detail.[5]

In a familiar fashion, this model divides the economy into three sectors (consumers, business firms, and government) and four markets (labor services, current output, bonds, and money). Two additional notions crucial to G-S's analysis are that of "nonfinancial spending units" and "primary securities." By the former is meant "spending units whose principal function is to produce and purchase current output, and not to buy one type of security by issuing another" (p. 59). Correspondingly, "primary securities" are defined as "all liabilities and outstanding equities of nonfinancial spending units" (*ibid.*). All consumers and firms in the preceding model are assumed to be nonfinancial spending units. Only firms, however, are assumed to issue primary securities, and these for simplicity are assumed to have the form of a homogeneous perpetuity paying $1 annually, so that its price is the reciprocal of the rate of interest, or $\$1/i$.

Since money is a type of security, and since the government (assumed to consist of both a Policy and a Banking Bureau) can acquire primary securities by issuing money, it is not a spending unit but a financial intermediary. Correspondingly, the debt of government, like that of any such intermediary, is referred to as an "indirect security." In this simple model the only such security is money. The term "financial asset" refers to both direct and indirect securities. Purchase of primary securities by consumers is referred to as "direct finance"; purchase by financial intermediaries (government, in this case), as "indirect finance" (pp. 59-61; 93-95).[6]

Each of the three sectors has a budget restraint, showing the sources and uses of its funds: this is essentially what is shown by the flow-of-funds account (p. 22). Each of these restraints states that the excess of receipts over spending (on capital as well as current account) must equal the sector's net accretion of financial assets (bonds and money). Every economic unit is classified as "surplus" or "deficit" according to whether this excess is positive or negative (p. 21). For the most part, households are surplus units and firms deficit units.

The nature of the relationships among the sectors of this economy can then be illustrated by the sectoral balance sheets of Table 1—where the symbols M^b, B, M^h, B^h, B^g and M are defined as indicated. From the definition given at the beginning of this section, it is clear that the economy described by this table has 100 units of inside money (corresponding to the value of the bonds held by the government and monetary sector) and 75 of outside money (cor-

[5] What the following actually describes is G-S's "modified second model" (pp. 82 ff.). This includes features present only in the "rudimentary model" of Chapter 2.

[6] Actually, the classification of the government is ambiguous, though G-S do not recognize it as such in their discussion on page 94. To the extent that it issues inside money, it is a financial intermediary. But to the extent that it issues fiat outside money to cover a deficit on current account, it is just like any other nonfinancial spending unit.

TABLE 1—SECTORAL BALANCE SHEETS

Business Sector				Consumer Sector			
Assets		*Liabilities*		*Assets*		*Liabilities*	
Money (M^b)	50	Bonds (B_0)	500	Money (M^h)	75	None	
Tangible	900			Bonds (B_0^h)	400		
		Net Worth				*Net Worth*	
		Accumulated Savings	450			Accumulated Savings	475
	950		950		475		475

Government and Monetary Sector

Assets		*Liabilities*	
Bonds (B_0^g)	100	Money (M)	175
		Net Worth	
		Accumulated Savings	− 75
	100		100

responding to the fiat money issued to finance the cumulated deficit of this sector).

In order to evaluate G-S's further analysis of this model, it will first be necessary to depart somewhat from their description of the demand functions. In particular, assume for simplicity's sake, and in accordance with recent developments,[7] that the household demand functions depend on its total wealth. The essential point for monetary theory is that this wealth also consists of the household's initial holdings of financial assets. This, indeed, is the origin of the real-balance and real-indebtedness effects. In particular, it is assumed that these demand functions depend upon—in addition to the rate of interest, i—household wealth, W, where

$$W = T + \frac{\dfrac{B_0^h}{i} + M_0^h}{p}, \tag{1}$$

where T represents the discounted value of the income stream; p represents the price level; and where the subscript "0" indicates given, initial quantities. Since the present discussion is restricted to a stationary state, T is constant and so can be disregarded in what follows. In a similar way it is assumed that businesses' demand functions depend on:

$$\frac{-\dfrac{B_0}{i} + M_0^b}{p}.$$

Let us now make the further assumption that in the economy as a whole

[7] Milton Friedman, *A Theory of the Consumption Function*, Princeton 1957, pp. 7-10.

there are no distribution effects; that is, that the aggregate demand of the economy depends only on the total of assets, and not on their distribution as between households and businesses. This means that these functions depend upon:

$$(2) \qquad \frac{\left(\frac{B_0^h}{i} + M_0^h\right) + \left(-\frac{B_0}{i} + M_0^b\right)}{p}.$$

Making use of the definitions:

$$(3) \qquad M_0 = M_0^h + M_0^b, \quad \text{and} \quad B_0 = B_0^h + B_0^g,$$

we can reduce this to:

$$(4) \qquad \frac{(M_0^h + M_0^b) + \frac{1}{i}(B_0^h - B_0)}{p} = \frac{M_0 - \frac{1}{i}B_0^g}{p}.$$

This last is clearly identified as the net real debt of the government sector to the private sector, or alternatively as the real value of outside money.

Making use of the preceding, let us write the following demand and supply functions:

$$(5) \qquad E = F\left(Y_0, i, \frac{M_0 - \frac{1}{i}B_0^g}{p}\right)$$

= aggregate demand for commodities;

$$(6) \qquad \frac{B^d}{rp} = H\left(Y_0, i, \frac{\frac{B_0^h}{i} + M_0^h}{p}\right)$$

= households' demand for real bond holdings as a stock;

$$(7) \qquad \frac{B^s}{rp} = J\left(Y_0, i, \frac{-\frac{B_0}{i} + M_0^b}{p}\right)$$

= businesses' supply of real bond holdings as a stock;

$$(8) \qquad \frac{M^d}{p} = L\left(Y_0, i, \frac{M_0 - \frac{1}{i}B_0^g}{p}\right)$$

= aggregate demand for real money holdings as a stock.

The foregoing system of equations has ignored the market for labor, for this is assumed to be in full-employment equilibrium (pp. 10, 26). In order to keep the discussion here related to the G-S one, the functions are also as-

sumed to depend on national income, Y, though the assumption that they depend on wealth might be taken to imply that this additional dependence on Y is otiose. Since in any event Y is assumed to be constant at Y_0, this issue will not affect the subsequent argument.

Assume that all demands are positively dependent on income[8] and net financial assets. On the other hand, business supply of bonds is negatively dependent on these variables; for the higher are a business' financial assets, the less it need be dependent on debt financing. What must now be emphasized is that the assumed absence of distribution effects implies that if households' financial assets increase by the same amount that firms' decrease, then the former's increased demand for bonds is exactly offset by the latter's increased supply. That is, the absence of distribution effects implies that at the level of aggregate behavior it is only the sum total of financial assets in the economy that matters. Hence the private sector's *excess* demand function for bonds—defined as the households' demand minus firms' supply—must have the form:[9]

$$(9)\quad B\left(Y_0, i, \frac{M_0 - \frac{1}{i}B_0^e}{p}\right) \equiv H\left(Y_0, i, \frac{\frac{B_0^h}{i} + M_0^h}{p}\right) - J\left(Y_0, i, \frac{-\frac{B_0}{i} + M_0^b}{p}\right).$$

On the basis of the foregoing discussion we can now write our general-equilibrium equations as:

Equilibrium Condition *Market*

$$(10)\quad F\left(Y_0, i, \frac{M_0 - \frac{1}{i}B_0^e}{p}\right) - Y_0 = 0 \qquad \text{commodities,}$$

$$(11)\quad B\left(Y_0, i, \frac{M_0 - \frac{1}{i}B_0^e}{p}\right) + \frac{B_0^e}{ip} = 0 \qquad \text{bonds,}$$

$$(12)\quad L\left(Y_0, i, \frac{M_0 - \frac{1}{i}B_0^e}{p}\right) - \frac{M_0}{p} = 0 \qquad \text{money.}$$

If all money is of the outside variety, then $B_0^e = 0$, and the foregoing system reduces to a very familiar form. G-S, however, are primarily interested in

[8] For reasons which are never explained G-S assume that the income elasticity of demand for money is greater than that for bonds (p. 71). This unexplained assumption is an important component of their subsequent argument (pp. 157-58).

[9] In mathematical terms, absence of distribution effects implies that $H_3(\ ) + J_3(\ ) \equiv 0$, where $H_3(\ )$ and $J_3(\ )$ represent the partial derivatives with respect to the third argument.

the opposite case, in which money is entirely of the inside variety. This means that:[10]

$$(13) \qquad M_0 = \frac{B_0^g}{i},$$

which in turn implies that the net financial assets of the private sector are zero. Hence system (10)-(12) reduces to:

$$(14) \qquad F^*(Y_0, i) - Y_0 = 0,$$

$$(15) \qquad B^*(Y_0, i) + \frac{M_0}{p} = 0,$$

$$(16) \qquad L^*(Y_0, i) - \frac{M_0}{p} = 0,$$

where $\frac{M_0}{p}$ simultaneously represents the government's real holdings of bonds and the total real quantity of money in the economy—and where the asterisks remind us that these functions differ from those of system (10)-(12).

The foregoing analysis has made use of what G-S call the "net-money doctrine": the "approach to monetary theory [which] nets out all private domestic claims and counterclaims before it comes to grips with supply and demand on the money market" (p. 134). This they contrast—very unfavorably—with the "gross-money doctrine" which "avoids such consolidation of financial accounts" (*ibid.*). Thus in the case of Table 1, according to G-S, the net-money doctrine would say that the quantity of money is 75, while the gross-money doctrine would say that it is 175 (pp. 134-36). Against the net-money doctrine G-S then bring to bear a detailed and elaborate criticism (pp. 134-49). But as we shall now see, this criticism is quite beside the point and stems simply from a misunderstanding of the fundamental distinction between dependent and independent variables of the analysis, a misunderstanding which mars G-S's argument at other points in their book as well.[11] In particular, G-S fail to distinguish properly between bond and money holdings as dependent variables (B^d, B^s, and M^d) and these holdings as independent variables (B_0^h, B_0, M_0^h and M_0^b).[12]

More specifically, G-S fail to realize that even though it is the ***net aggregate value*** of financial assets which is the proper ***independent*** variable of the demand functions of the private sector, it is the ***individual financial assets*** which are the proper ***dependent*** variables of the analysis. Thus in the special case of all money being inside money, system (14)-(16) shows that net financial assets disappear as arguments (i.e., ***independent*** variables) of the demand

[10] Strictly speaking, the following relationship implicitly assumes that all government capital gains from a reduction in interest are returned to the private sector as transfer payments financed by printing money. A corresponding statement holds, *mutatis mutandis*, for capital losses.

[11] See, for example, top of page 29; see also the discussion in footnote 20, below.

[12] This distinction is quite clearly made in the Mathematical Appendix, especially on page 320. But unfortunately there is no communication between the appendix and the text on this crucial point.

functions; but this in no way affects the fact that, in back of system (14)-(16), lies a system like (5)-(8) in which the individual demands and supplies for financial assets remain the *dependent* variables of the analysis. Though it is necessarily the net-money doctrine which is relevant for considerations of the wealth effect on demand functions, it is equally necessarily the gross-money doctrine which is relevant for considerations of the optimum amount of money which an individual wishes to hold in his portfolio. There is no contradiction between these two "doctrines": they simply refer to two completely different things.[13]

It should be emphasized that there is nothing unusual about this distinction: thus, consider the familiar case of an economy consisting of individuals who receive their income in the form of initial endowments of two commodities, x and y. If we abstract from distribution effects, the demand functions of this economy depend upon (among other things) the *aggregate* value of the initial quantities of x and y (i.e., total income); but this in no way affects the fact that the economy has *separate* demand functions for x and y. This example brings out the further point that the net-money doctrine does not represent an additional and optional assumption, but is the logical consequence of assuming at one and the same time that (a) the demand of an individual depends on his total wealth and (b) there are no distribution effects. G-S explicitly accept the second of these assumptions—and give no indication of rejecting the first. Hence, if they are to be consistent, they themselves cannot reject the net-money doctrine.

From all this it is clear that, contrary to the contentions of G-S, the net-money doctrine does *not* imply that an economy with only inside money is "money-less and bond-less" (p. 138), so that its price level is indeterminate (pp. 142-44). Indeed, the determinacy of such a system is immediately evident from equations (14)-(16) above. For though an arbitrary (say) increase in p will not affect any of the *demand* functions in this system, it will, by decreasing $\dfrac{M_0}{p}$, create an *excess* (or, in G-S's terminology "incremental") demand for money; alternatively, it will create an excess supply of bonds. This will increase i, thereby [from (14)] create an excess supply of commodities, and thereby drive the price level down again.

This is how the "traditional argument" would be stated. It can also be stated, in G-S's conceptual framework, in terms of the fact that the increased price level disturbs the portfolio equilibrium of the private sector by decreasing its real money holdings relative to its bond holdings, and that the attempt of the private sector to re-establish equilibrium will cause the system to return

[13] At one point (p. 320, top), the Mathematical Appendix does write the demand functions as dependent upon total financial assets. But this procedure is not carried over to other parts of the appendix—and its implications for the argument of the text are not seen.

On my own confusion on related points in earlier writings, see footnote 3 above.

[14] Pp. 74-75, 79, and 143. There is some confusion in G-S's presentation of the argument. For they assume that the amount of bonds firms supply goes up proportionately with the price level, and state that such an assumption is necessary in order "to avoid distribution effects" (p. 75). Now, first of all, by G-S's own assumption as to the be-

to its original position.[14] This may be a more sophisticated way of describing the matter, but it certainly does not differ substantively from the argument of the preceding paragraph. And there is certainly no basis for G-S's contention that the "net-money doctrine overlooks the bearing of portfolio balance on real behavior" (p. 144).

I would conjecture that what misled G-S in all this discussion is the fact that the *demand* functions of a purely inside-money economy are independent of the absolute price level (note their emphasis on this fact on pp. 73-74). From this (I suspect) they incorrectly inferred that the system of equations of such an economy is ensnared in the price indeterminacy of the invalid dichotomy. They did not realize that what involves a system in such a dichotomy is instead the quite distinct assumption that all (nonmoney) *excess demand* functions are unaffected by the price level. And they particularly did not realize that, as shown by equations (14)-(16), a purely inside-money economy is actually a particular instance of the special case in which the system can be validly dichotomized by virtue of the fact that, though the commodity excess-demand equation is independent of the price level, the bond excess-demand equation is not.[15]

Looking at the inside-money model in this way also enables us quite simply to deduce one of G-S's main conclusions: namely, that open-market purchases in such a model (i.e., government acquisition of private securities) will not affect the rate of interest. Equation (14) shows us that, under the assumption of full employment, there is only one rate of interest at which the commodity market can be in equilibrium. Hence nothing that happens in the bond or money markets alone can affect the equilibrium rate of interest. Once again, we can equivalently carry out the argument—as G-S do—in terms of "portfolio balance" (pp. 76-77). But in this case I believe that the G-S argument is less revealing than the "traditional one": for it fails to bring out the crucial nature of the dichotomy between the real sector and monetary sector which is implicit in the assumption of a purely inside-money economy.

By way of contrast, consider the case in which there is both inside and outside money, so that the more general system (10)-(12) obtains. It is immediately evident that the commodity market in this case can be in equilibrium at an infinite number of combinations of interest rate and price level. Hence the

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havior of firms (pp. 63-64), an increase in the price level will *not* cause a proportionate increase in the supply of bonds—for it will have an encouraging "debt effect." Secondly, as shown above, what is necessary for the argument is that the *excess* demand function for bonds be free of distribution effects—and this can obtain even if the amount of bonds outstanding remains constant. For, as emphasized in equation (9) above, all that absence of distribution effects means is that the indebtedness effects of firms and households offset each other.

This confusion recurs throughout G-S's book (cf., e.g., pp. 88, 234 and 251). They may, of course, be thinking of changes in the *initial* quantities of bonds. But since, as just emphasized, they do not distinguish properly between initial and demanded quantities, their argument is not clear.

[15] See *Money, Interest, and Prices*, pp. 109-10. The validity of dichotomizing the system in this case has recently been emphasized by Franco Modigliani in the postscript attached to the reprinting of his well-known article on "Liquidity Preference and the Theory of Interest and Money" in *The Critics of Keynesian Economics*, ed. H. Hazlitt, Princeton 1960, pp. 183-84.

equilibrium condition in this market does not uniquely determine the rate of interest, so that there is room for changes in the interest rate to result from changes originating in the bond or money markets (the monetary sector). In particular, an increase in outside money in this case will *raise* the interest rate (pp. 85-86)[10] while an increase in inside money will *lower* it (pp. 84-85, 144-47). On the other hand—as the reader can readily verify from system (10)-(12)—a proportionate increase in both inside and outside money will cause a proportionate increase in prices and leave the interest rate invariant (p. 85).

Two comments must be made here. First—and related to our earlier discussion—the open market purchase of bonds in the preceding case does not *initially* (i.e., before the interest rate change) affect the net obligation of the government to the private sector as represented by equation (4) above; correspondingly, it does not initially affect the *demand* functions in system (10)-(12). But by generating corresponding increases in B_0^g and M_0, it does create an *excess* demand for bonds and an *excess* supply of money—thus driving the interest rate down. In this way we see the untenability of G-S's contention that the "net-money doctrine . . . implies that management of inside money cannot come to grips with the rate of interest" (p. 147).

My second comment is more a question of emphasis. G-S have done a real service in making explicit the distinction between inside and outside money. At the same time I think it is unfortunate that they have chosen to refer to the foregoing results as proving "that money is not neutral, within a neo-classical framework, when there is a combination of inside and outside money" (p. 232). For G-S repeatedly recognize that the "neo-classical framework" implies the absence of distribution effects—which means, among other things, that the increased money supply is injected into the system so as not to disturb "the pattern of demand for current output" (p. 41). Why, then, is there any essential difference between this and assuming that the money is injected so as not to disturb the pattern of demand for financial assets? And is it not this latter condition that we are implicitly fulfilling when we assume that the monetary increase is accomplished in such a way as not to disturb the proportions between inside and outside money?

Before concluding this section I must voice a more fundamental objection, though not to an analytical aspect of the book. G-S have explicitly adopted the procedure of "no footnotes and no bibliography" (p. x). There is much to be said for such a procedure. But it would seem to me that the "ground rules" (to use one of the G-S's pet phrases) of scholarship then require the authors to foreswear the pleasures of *Dogmengeschichte*, and particularly the pleasures of casting anonymous shafts at "traditional" and "neo-classical" economics. Unsupported *obiter dicta* on the nature of the latter can only add confusion to an already complicated issue.

Thus I for one would have been much happier not to have been unequivocably told, with respect to an economy all of whose money is of the inside variety, that "the traditional answer would be that the price level is not

[10] This rather surprising conclusion results from the assumption that the government is a creditor of the private sector. If it were a debtor, interest would fall. See Enthoven's analysis, pp. 330-33. See also *Money, Interest, and Prices*, ch. 12, sec. 5, and Mathematical Appendix, 9: e.

determinate, and that any price level would be compatible with general equilibrium" (p. 74); or that "the quantity-theory solution"—for the case of an increase in inside money when there exists money of both varieties—is "a new equilibrium at doubled levels of commodity prices, money wage rates, and nominal primary securities—with the rate of interest unchanged" (p. 145; see also p. 147). And I would have been happiest to have been informed who were the culprits responsible for such statements, and how far back in the literature one has to go in order to find signs of their activity.

III. *The Influence of Financial Intermediaries*

Within the confines of one book G-S have admirably transformed the analysis of financial intermediaries from a hitherto neglected question in the literature to one which will now have a recognized place in the corpus of monetary theory. My only objection to their analysis is that it is not sufficiently integrated into this corpus, and that as a result its full significance is not brought out. This is what will now be shown. At the same time it should be made clear at the outset that from the substantive viewpoint the following merely repeats G-S's analysis. It gives it only a slightly different twist—a slightly different emphasis. But it seems to me that this emphasis is of importance in simplifying and clarifying the analysis.

G-S fruitfully conceive of financial intermediaries (especially of the nonbanking type) as processing plants whose function it is to "turn primary securities into indirect securities for the portfolios of ultimate lenders" (p. 197). Intermediaries are able to profit by this transformation process by exploiting "economies of scale in lending and borrowing. On the lending side, the intermediary can invest and manage investments in primary securities at unit costs far below the experience of most individual lenders. The sheer size of its portfolio permits a significant reduction in risks through diversification. It can schedule maturities so that chances of liquidity crises are minimized. The mutual or cooperative is sometimes favored with tax benefits that are not available to the individual saver. On the borrowing side, the intermediary with a large number of depositors can normally rely on a predictable schedule of claims for repayment and so can get along with a portfolio that is relatively illiquid" (p. 194).

In other words, the result of developing nonbanking financial intermediaries (like that of improving distributive techniques) is to provide ultimate lenders with the possibility of purchasing a security which is more attractive (more "liquid") than the primary securities issued by the ultimate borrowers (pp. 123-26). If G-S had followed up this aspect of their argument, they could have simply and instructively presented it as analytically equivalent to the case of an assumed increase in the liquidity of bonds within a standard Keynesian model. Such an increased liquidity makes bonds a better substitute for money, and thus causes the demand curve for money both to shift leftwards and to become more elastic. This is represented by the shift from D to D' in Figure 1, which essentially reproduces G-S's Charts 5 (p. 163) and 8 (p. 216). It follows that if the real supply of money remains constant at OC, then the rate of interest must decline from i_1 to i_0.

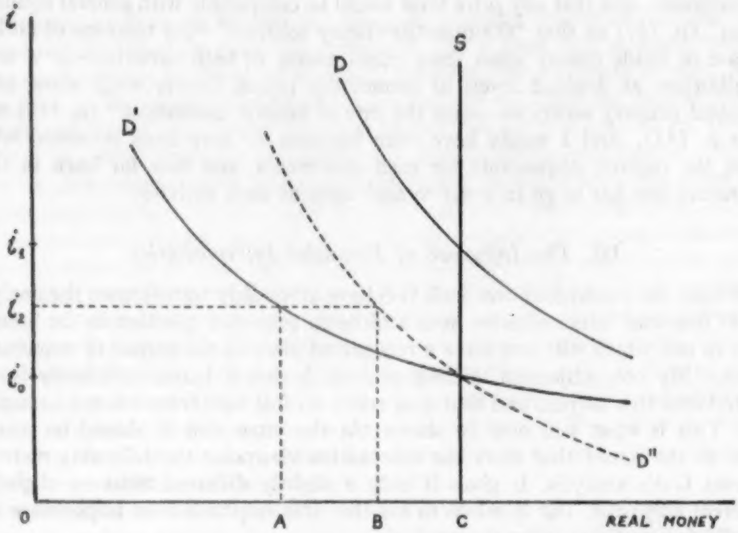

FIGURE 1

This, within the familiar framework of Keynesian theory, is the why and wherefore of the influence of financial intermediaries on the rate of interest, and thence on the real variables of the system. And within this same familiar context G-S could also have simply, succinctly, and systematically placed their analysis of an assumed greater marketability of securities, or smaller risks of default, or a shift in the "mix" of securities in favor of shorter-term obligations (pp. 160-73); or of government's substituting its securities for private ones, or insuring the latter (pp. 224-27); or of government's decreasing the liquidity of bonds relative to money by insuring the latter (pp. 225-26; this, of course, would reflect itself as an opposite shift—from D' to D). In a similar way, we can see from the diagram that if the monetary authority wishes to raise the interest rate from i_0 to i_2 by open-market sales, then it must decrease the real money supply much more in the case of the elastic demand curve D', which characterizes a system with nonbanking financial intermediaries, than in the case of the less elastic curve D'', which characterizes a system without them (pp. 239-41, 286).

Note that implicit in the foregoing exposition is the assumption that financial intermediaries are just that: namely, that they do not themselves change their money holdings in carrying out the full cycle of their activities. This, it seems to me, is the way the analysis should be carried out. Correspondingly, it also seems to me that G-S's attempt to achieve formal completeness of the argument by using as one of their standards of comparison a case in which intermediaries hoard money (pp. 217-18) simply confuses the nature of the real issues involved.

Finally, I should emphasize that in order to keep the foregoing exposition as close as possible to G-S's, I have carried it out in terms of the demand for and supply of money. Actually, however, I would have perferred to have carried it out in terms of a simultaneous general-equilibrium analysis of the commodity, bond, and money markets together. In particular, a parameter, t, can be introduced into each of the demand functions in system (10)-(12) to represent the degree of liquidity of bonds. An increase in t can be assumed to increase the demand for bonds, decrease that for money, and have either a positive or negative effect on that for commodities. This would have insured that we would not lose sight—as G-S sometimes do[17]—of the concomitant happenings in the commodity market as changes occur in those for bonds and money.

In this way it would also have been possible to bring out G-S's frequently emphasized point of the essential similarity between banking and nonbanking intermediaries (see Section I above). To speak somewhat loosely, both of these intermediaries influence the economy by affecting the terms on which bonds are demanded and supplied. But, in terms of the bond equation (11) above, the banking system does this by (say) increasing the second component of this demand $\left(\dfrac{B_0^g}{ip}\right)$; whereas nonbanking intermediaries, by increasing the liquidity of bonds, and hence t, do so by increasing the first component $[B(\quad)]$. Alternatively, in somewhat more familiar terms, we can make the comparison in terms of the money equation (12) and say that the banking system affects the economy by changing the supply of money $\dfrac{M_0}{p}$; whereas nonbanking intermediaries do so by changing t, hence the demand for money $L(\quad)$, and hence the velocity of circulation. Note, however, that in both cases there are further endogenous changes in velocity caused by the change in the interest rate.

IV. *The Minimum Requirements of a Monetary System*

G-S have reserved their most provocative question of all for their concluding chapter. Here they ask: What are the minimum prescriptions a monetary authority must set in order to assure the determinacy of the price level in a monetary system? Alternatively, and somewhat more pointedly, I would prefer to ask the question in the following way: In most discussions of monetary theory the nominal quantity of money supplied is taken as an exogenous variable. But though we continuously shy away from this fact in our theoretical work, we do nevertheless know that in the real world this is not the case:

[17] I am thinking primarily of G-S's discussion on pages 153-58, which is based on the tacit assumption that the total demand for financial assets remains fixed. This discussion is then used as the basis for analyzing changes in the maturity of bonds, in their degree of risk, and so forth (pp. 159 ff.)—despite the fact that all of these changes will in general also affect the demand for commodities and hence the total demand for financial assets.

Note, too, that in their discussion of financial intermediaries, G-S explicitly assume "that there is no change in spending units' demand for current output" (p. 214, n. 3).

for money is largely the creature of a banking system which responds to such endogenous variables as the rate of interest, the wages of clerks, and so forth. How then can we take account of these responses? And, in particular, is there a limit to the extent to which such endogenous influences can be assumed to operate? Conversely, must a determinate monetary system necessarily retain some exogenous element?

One cannot read G-S's concluding chapter without being struck by the success with which the authors have refreshingly broken through the traditional confines of thought, and by the fruitfulness with which they apply their analytical apparatus to problems of banking theory. But once again their exposition is marred by imprecisions and errors, and by a consequent failure to see the full and deeper significance of the argument.

G-S's discussion of this question is carried out almost entirely in terms of the purely inside-money model. They begin by introducing two new assumptions: first, that the banking system pays an "explicit deposit rate," d, on the money which it has issued (e.g., on its demand deposits); second, that the behavior of the banking system, like that of any other sector, is free of money illusion—i.e., that it is determined only by real variables (pp. 248-57).

Introducing the first of these changes into the inside-money system (14)-(16) above converts it into:

$$(17) \qquad F(Y_0, i, d) - Y_0 = 0 \qquad \text{commodities,}$$

$$(18) \qquad B(Y_0, i, d) + \frac{M_0}{p} = 0 \qquad \text{bonds,}$$

$$(19) \qquad L(Y_0, i, d) - \frac{M_0}{p} = 0 \qquad \text{money.}$$

In other words, in formulating their demand decisions in this economy, individuals take account of the rate of return on the asset money (d), as well as the rate of return on the asset bonds (i).

It is immediately evident that the foregoing system is indeterminate. For assume that we begin from an initial equilibrium position with d at a certain level, and that d is then increased. This is analytically equivalent to an increase in liquidity preference: money balances become more desirable at an unchanged rate of interest. Hence the system will reach a new equilibrium position which will reflect this shift in liquidity preference. It will not return to the original equilibrium position (pp. 248-52).[18]

G-S, however, assume that the new equilibrium position differs from the original one only in the price level, while the rate of interest remains the same. This results from their assumption that the foregoing shift in liquidity preference is a neutral one as between bonds and commodities (p. 252). This assumption, however, is quite unreasonable within the given context. For the change in d primarily affects the relative desirability of bonds and money as liquid assets. Correspondingly, the shift in liquidity preference which takes place is primarily at the expense of bonds—and little, if at all, at the expense of commodities. Hence such a shift causes a permanent increase in the interest

[18] In technical terms: system (17)-(19) has three variables—i, d, and p, and (by Walras' Law) only two independent equations. Hence it is generally indeterminate.

rate,[19] even when money is entirely of the inside variety. G-S do recognize this as a possibility (p. 252, bottom); but they do not see that this is what should normally be expected to happen.

Let us now proceed to G-S's second change—namely, that the quantity of money is no longer the exogenous variable that it is in system (14)-(16), but an endogenous one determined by the profit-maximizing behavior of the banking system. In brief, we now have a money-supply function of the banking system (which is simultaneously its bond-demand function) that states (and here I differ from G-S[20]):

$$\frac{M}{p} = S(i, d). \tag{20}$$

More specifically, the real amount of money the banking system wishes to supply (the real amount of bonds it demands) depends directly on the interest rate it receives and inversely on the deposit rate it pays. Substituting this into (17)-(19), we obtain:

$$F(Y_0, i, d) - Y_0 = 0, \tag{21}$$

$$B(Y_0, i, d) + S(i, d) = 0, \tag{22}$$

$$L(Y_0, i, d) - S(i, d) = 0. \tag{23}$$

Now, it is reasonable to assume that this system of two independent equations can determine the equilibrium values of its two variables, i and d. However, it clearly cannot determine the equilibrium price level (p. 255). This is obvious from the fact that p does not even appear as a variable of the system. Due to the incorrect way in which they write the money-supply function—a way in which p does ostensibly appear in the function[21]—G-S do not realize this crucial fact. Nor, accordingly, do they realize the full significance of the foregoing indeterminacy. This, in brief, is that in order for the absolute price level to be determined by market-equilibrating forces, changes in it must impinge on aggregate *real* behavior in *some* market—i.e., must create excess demands in some market. Now, the joint assumptions of a purely inside-money and the absence of distribution effects implies that there is no such impingement on the real demands of the private sector for commodities, bonds, or money, respectively. Similarly, we have assumed that there is no impingement on the real demand and supply functions of the banking sector. Hence the economy does

[19] *Money, Interest and Prices*, pp. 169-70.

[20] G-S write this function (p. 254) as:

$$\frac{M}{p} = S\left(\frac{B^e}{ip}, i, d, \frac{w}{p}\right).$$

For simplicity, the argument w/p—representing the real wage rate—has been omitted here in accordance with the assumption (made by G-S as well) that the labor market is always in equilibrium. On the other hand, G-S's inclusion of B_e/ip as an argument of the function is not explained by them—and seems to me again to reflect their failure to distinguish properly between dependent and independent variables (see Section I above). For by the inside-money assumption, $B^e/ip = M/p$, so that what G-S have essentially done is to write a supply function in which the same variable, M/p, appears simultaneously as a dependent and independent variable! This confusion has consequences for their later argument which will be noted in a moment.

[21] See preceding footnote.

not generate resistance to any arbitrary change in the price level. Accordingly, there is nothing to prevent the frictionless flow of prices from one level to another.

G-S now move one step closer to reality and assume that there is a central bank which creates reserves solely by purchasing primary securities. Reserves are thus a type of security issued by one financial intermediary (the central bank) and, by assumption, acquired only by another (member banks) (pp. 257-58). In terms of the sectoral balance sheets of Table 1 above, this means that though there is no effect on the household and business sectors, the government and monetary sector is now divided into two sectors, as in Table 2.[22]

TABLE 2—SECTORAL BALANCE SHEETS—MEMBER BANKS AND CENTRAL BANK

Member Banks				Central Bank			
Reserves	25	Money (demand deposits)	100	Bonds (primary securities)	25	Reserves	25
Bonds (primary securities)	75						

It is further assumed that like any other security, reserves, too, provide a rate of return, namely, the "reserve-balance rate," d'. It is first assumed that member banks are not subjected to reserve requirements. Instead, they are free to choose their optimum portfolio of assets and liabilities—including, of course, reserves. Like any other economic unit, the private banking system's optimum portfolio depends only on real variables—and, in particular, on the alternative rates of return of the three assets (and liabilities) on which it must formulate its decisions: namely, i, d, and d'. Thus the private banking system's real supply function of money is now assumed to be:

$$S(i, d, d'), \tag{24}$$

and its real demand function for reserves,

$$G(i, d, d').\text{[23]} \tag{25}$$

At the same time, the private banking system's supply of money is clearly no longer identical with its demand for bonds. The latter is instead now represented by, say $U(i, d, d')$ where, by the banks' balance sheet of Table 2,

$$U(i, d, d') \equiv S(i, d, d') - G(i, d, d'). \tag{26}$$

On the other hand, the central bank's real supply of reserves, which is also its real demand for primary securities, is:

$$\frac{R}{p}, \tag{27}$$

[22] Because of the assumption that all money is inside money, Table 2 disregards the 75 units of outside money in Table 1.

[23] Once again, these equations differ from those of G-S (pp. 258-59) for the same reason noted in footnote 20 above. Note in particular that G-S's money-supply function depends on B_e^g/ip, equal to members' reserves (p. 258). But, as we have just seen, this is quite wrong: for reserves demanded and money supplied are both dependent variables—dependent on the same three variables, i, d, and d'.

where R is the nominal quantity of reserves. Correspondingly, system (21)-(23) becomes:

$$(28)\qquad F(Y_0, i, d) - Y_0 = 0 \qquad \text{commodities,}$$

$$(29)\qquad B(Y_0, i, d) + U(i, d, d') + \frac{R}{p} = 0 \qquad \text{bonds,}$$

$$(30)\qquad L(Y_0, i, d) - S(i, d, d') = 0 \qquad \text{money,}$$

$$(31)\qquad G(i, d, d') - \frac{R}{p} = 0 \qquad \text{reserves.}$$

G-S summarize their argument in the following terms: "Of three indirect techniques—fixing nominal reserves [R], setting the reserve-balance rate [d'], and setting members' own deposit rate [d]—the Central Bank can get along with any two in regulating all nominal variables in the economic system" (pp. 274-75); and G-S themselves concentrate on showing how the system works when the first two are chosen (pp. 259-63). At the same time they contend that a central bank that can only regulate the reserve-balance rate, d', is in a weaker position than a monetary authority that can "directly" regulate the deposit rate, d (pp. 263-64).

These passages reveal G-S's failure to understand two basic aspects of the argument. First, and most important of all, it is *not* a matter of indifference to the central bank as to which two of the three variables—R, d', or d—it chooses to fix. The decision on R is *not* analogous to the decision on d or d'. Indeed, unless the central bank makes a decision on R (though not necessarily the one indicated by *G-S*) it can not achieve a determinate price level.

In order to show this, let us for the sake of contrast first assume that the central bank, too, is influenced only by real variables. Then, instead of (27), its real supply function of reserves would be represented by, say,

$$(32)\qquad T(i, d').$$

Replacing $\frac{R}{p}$ by this expression in system (28)-(31) would then yield a system completely analogous to (21)-(23) above: namely, one that could determine the rates of return, but obviously not the price level, which would not even appear in it!

In terms of our earlier argument, all that this means is that since in the aggregate no other economic unit in system (28)-(31) reacts to changes in the absolute price level, then, in order to assure the determinacy of the price level, the central bank must do so. This is the behavior denied by equation (32) but affirmed by equation (27). For the latter implies that the supply of real reserves is inversely proportionate to the price level. On the other hand, it is clear that the central bank need not operate in this way. If it acts in accordance with any supply function for *real* reserves which is dependent on the absolute price level—say,

$$(33)\qquad W(i, d', p)$$

—the same purpose will be accomplished. In brief, a necessary and sufficient condition for the price level to be determinate in the foregoing inside-money

model is that the central bank be willing to suffer from money illusion in one form or another! This is the essential point.[24]

From this we move to our second criticism. Once it has decided to fix the level of nominal reserves [or, more generally, act in accordance with (33)], the central bank uniquely determines all the variables of the system by fixing in addition either one of the two rates d or d'—or for that matter (though G-S do not realize this) any one of the three rates d, d', or i. Hence at the theoretical level at which G-S are carrying on their discussion it cannot make a particle of difference which rate is so fixed. Accordingly it is meaningless to say that a monetary authority that can choose d is in a "stronger position" than one that can only choose d'.

The general conclusion that we can draw from all this is that, in the absence of distribution effects, the necessary conditions for rendering a monetary system determinate is that there be an exogenous fixing of (1) some nominal quantity and (2) some rate of return. It follows that if we were to extend the argument to an economy with both inside and outside money (something G-S do not do) it would suffice to fix the quantity of outside money and its rate of return (say, at zero). In such an economy the price level would be determinate even if the central bank were to fix nothing, and operate solely in accordance with the principle of profit-maximization [as represented, say, by equation (32)]—subject to the restriction that the quantity of outside money is fixed.

It is, therefore, unfortunate that G-S have so strongly tied their discussion of central bank policy with that of the conditions necessary to achieve a determinate price level. As can be seen from what has just been said, this is not the real issue at all. Instead, the analysis of central bank policy should concentrate on the behavior (obviously, of a non-profit-maximizing type) such a bank can adopt with reference to such decisions as (say) open-market purchases of bonds in order to bring about *desired* changes in a *determinate* price level and in other variables of the economic system. But it would carry us too far afield to explore these questions in any further detail here.

[24] This presentation enables us to solve a seemingly paradoxical aspect of G-S's argument. They emphasize that a system with competitive banks—but no central bank—is determinate in its rates of return but not in its price level. On the other hand, the introduction of a central bank renders the system indeterminate both in rates of return and the price level. This raises the puzzling question: Why should the introduction of one new equation (for reserves) and one new variable (d') make the indeterminacy so much greater?

But what we now see is that G-S's first system corresponds to (21)-(23), in which the "indeterminacy" of the price level is just a complicated way of saying that this variable does not appear in the system at all. Instead, there exists a system of two (independent) equations in two variables (i, d). And as just noted in the text, we would get exactly the same type of "indeterminacy" if we were to introduce the central bank in the illusion-free way described by equation (32).

On the other hand, once we assume that the central bank suffers from money illusion, we obtain a system like (28)-(31), consisting of three independent equations in four variables (i, d, d', and p), each of which actually appears in the system. And since there is no reason to assume that this system has a determinate subsystem, indeterminacy prevails in all the variables. This, of course, is also characteristic of system (17)-(19), to which system (28)-(31) is essentially similar.

COMMUNICATIONS

The Reform and Revaluation of the Ruble

This note is an analysis of the Soviet currency reform and exchange rate change effective on January 1, 1961.[1] An understanding of the currency reform is essential to a proper assessment of the revaluation as a depreciation, rather than as an appreciation (as claimed by Soviet officials), of the ruble.

I. *The Currency Reform*

The currency reform was announced by Khrushchev in a speech to the Supreme Soviet on May 5, 1960, and an official decree was published in the Soviet press the following day [9]. Beginning January 1, 1961, a new ruble currency will be issued at the rate of 1 new ruble for 10 old rubles.[2] Also, effective January 1, 1961, all prices, wages and other money incomes, taxes, savings deposits, government bonds, and assets and liabilities of Soviet enterprises are to be reduced to one-tenth of their former level.[3] The currency reform will thus have no real impact on the average Soviet citizen, whose real income and real asset positions will be unaltered as a result of the change, in the same proportion, of wages, prices, assets, and claims.[4]

In this respect, the 1961 reform differs from the early post-war monetary reform of December 1947. Under the latter, new currency was exchanged for old at the rate of 1-to-10; savings deposits were exchanged at varying rates, ranging from 1-to-1 for small deposits to 1-to-2 for large deposits; and government bonds, with some exceptions, were exchanged at a rate of 1-to-3. On the other hand, wage rates were unchanged while prices were reduced, although not in proportion to the reduction in the population's holdings of currency and near-money.[5] The primary objective of the 1947 reform was thus to wipe

[1] This note was written at the end of November 1960 and is based on information available at that time.

[2] The exchange of notes and coins is to be completed within three months, i.e., by April 1, 1961. In the interim, old notes and coins will be accepted for all payments at one-tenth their face value.

[3] Since the minimum unit will continue to be the kopeck, now the smallest coin in circulation, the new prices will be rounded up or down to the nearest kopeck. (100 kopecks equal 1 ruble.) For example, a 35 kopeck price will become 4 kopecks and a 34 kopeck price, 3 kopecks. As an exception to this general rule, all rounding will be downward in the case of certain mass consumption goods, such as bread, milk and dairy products, and selected articles for children.

[4] The Soviet currency reform thus resembles closely the French currency reform effective January 1, 1960, which substituted 1 new "heavy" franc for 100 old francs, and may have been inspired, at least in part, by the French reform.

[5] Concurrently with the monetary reform, rationing was ended, and new retail prices were fixed at a level intermediate between the previous low "ration" prices and the higher "commercial prices" at which off-ratio purchases could be made. The new retail price level

out inflationary pressure arising from large cash balances accumulated during the second world war as a result of wartime deficit finance and held to a large extent by peasants who had obtained high prices on the free market for scarce food during the war [12, pp. 478-80]. Real incomes were favorably affected, however, as a result of the general price reductions accompanying and following the 1947 monetary reform, while money wages were unchanged by the reform and rose gradually thereafter [8].

In contrast, the 1961 reform is not intended to eliminate currency hoards,[6] nor are general price reductions promised. Indeed, in the same speech in which the currency reform was announced, Khrushchev specifically reiterated the present Soviet policy of granting increases in real income primarily through selective increases in money income (and, through differential reductions in income taxes deducted at the source, in the take-home component of gross wages), while keeping the general level of retail prices relatively stable, as it has been since 1954 [9].

Thus the currency reform itself will have no significant impact on the Soviet population or on the Soviet economy. Surely the claimed advantages from the simplification of bookkeeping, greater attention to economizing every ruble and kopeck, and reduced expenditures for printing money (aside from the necessary initial issue) [9] are of minor significance. Although there may be some favorable psychological impact of a "more valuable" ruble on the population and on enterprise managers and workers, its importance is questionable.

II. *The Revaluation of the Ruble*

The establishment of a new gold content and exchange rate for the ruble was decreed on November 14, 1960 [11], six months after the announcement of the currency reform, although it was foreshadowed by a clause in the May currency reform decree stating that the Ministry of Finance, the State Planning Committee, and the State Bank had been "instructed to draft proposals for establishing a new rate of exchange between the ruble and foreign currencies and for raising the gold content of the ruble" [9].

On February 28, 1950, the USSR had fixed the gold content of the ruble at .222168 grams of fine gold. With the par value of the U.S. dollar fixed

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was apparently below the weighted average of the "ration" and "commercial" prices, although the available data are not very clear on this point [8, pp. 39, 46]. Retail prices were subsequently reduced in the spring of 1948, and each spring thereafter through 1954, as output and supplies of consumer goods increased. However, wholesale prices of consumer goods and prices of producer goods were not altered until 1949, and agricultural procurement prices were not substantially changed until 1953.

[6] Although new currency is to be exchanged for old without limit at the specified rate, the reform will strike at those who have accumulated large currency hoards through "speculation" (i.e., black market) and other illegal activities and who will be loath to present them for exchange, because they will be unable to account satisfactorily for the size of their currency holdings. According to the Soviet press, such people have been endeavoring to convert their currency hoards into other assets, such as foreign currencies purchased from foreign visitors, precious metals, and jewelry. Thus, for instance, the Soviet jewelry trade organization is reported to have fulfilled its annual sales plan for 1960 in less than seven months. [14] Likewise, the comparatively small amount of ruble notes held outside the Soviet Union will become invalid as a result of the currency reform.

at .888671 grams of fine gold (equivalent to $35.00 per troy ounce of fine gold), the exchange rate between the ruble and the dollar was accordingly set by the Soviet government at 4 rubles per dollar or $0.25 per ruble. As a result of the 1961 currency reform, by itself, the gold parity for the new ruble should have been set at 10 times the gold parity of the old ruble, given the conversion ratio of 1 new for 10 old rubles. Thus, the gold parity should have been 2.22168 grams of fine gold per ruble, yielding an exchange rate with the dollar of of 40 kopecks per dollar or $2.50 per ruble. Instead, however, under the November 14, 1960 decree, the gold parity of the ruble is set, effective January 1, 1961, at .987412 grams of fine gold, and the exchange rate between the ruble and dollar, correspondingly, at 90 kopecks per dollar or $1.11 per ruble. Thus, the gold parity and exchange rate with the dollar have been fixed at 44.4 per cent of the level which the currency reform alone should have produced, representing a sharp devaluation of the ruble, rather than an appreciation, as claimed in official Soviet statements (e.g., by Finance Minister Garbuzov [11]).[7]

There is, however, one respect in which the ruble will in fact be appreciated on January 1, 1961. In addition to the basic exchange rate of 4 rubles per dollar, used for the conversion of foreign currency in merchandise transactions, the Soviet State Bank also established, on April 1, 1957, a special rate of 10 rubles per dollar (and corresponding rates with other currencies) for "noncommercial" transactions, such as expenditures of diplomatic missions; the sale of rubles, in exchange for foreign currency, to foreign travelers visiting the Soviet Union; and the sale of foreign currency, in exchange for rubles, to Soviet citizens traveling abroad [13, pp. 98-99]. As a result of the currency reform alone, the corresponding rate should have become 1 ruble per dollar, given the conversion ratio for exchanging new for old rubles. Instead, effective January 1, 1961, this special "premium" rate will be abolished, and these transfers will be made at the new single rate of 90 kopecks per dollar or $1.11 per ruble. Thus, for these transactions, the combination of the currency reform, the exchange-rate change, and the elimination of the special rate will cause an 11 per cent appreciation of the ruble.

This limited appreciation of the ruble constitutes the only immediate economic effect of the revaluation of the ruble, which will have no real economic impact either on Soviet foreign trade or on Soviet citizens and enterprises. There will be no direct impact on foreign trade because Soviet foreign trade is conducted at (or, as a result of bargaining, on the basis of) world market prices expressed in international currencies such as the dollar and the pound sterling. For statistical purposes, however, trade figures expressed in these foreign currencies are "translated" into rubles at the officially specified exchange rate, which on January 1, 1961, becomes 90 kopecks per dollar instead of 4 rubles per dollar. But the revaluation will not directly affect the volume, composition, or direction of Soviet foreign trade. Likewise, Soviet domestic prices will not be affected, because imports are sold within the USSR at internal ruble prices fixed in relation to the price structure for domestically

[7] This devaluation will not, however, affect Soviet foreign trade for reasons explained below.

produced goods—rather than being sold at the ruble equivalent, at the official exchange rate, of the foreign price [7, pp. 435-36]. Hence, the depreciation of the basic exchange rate will not affect the Soviet trade balance nor the Soviet internal price level. The appreciation of the ruble in relation to those "invisible" transactions which formerly enjoyed the special favorable rate may tend to reduce foreign exchange earnings from this source, but it is not likely that the impact will be substantial.

A prestige or propaganda gain appears to be one of the major benefits sought through the revaluation. In explaining the revaluation [11], Finance Minister Garbuzov asserted that "in recent years the reputation of the dollar has been seriously undermined" by the United States' adverse balance of payments, the accumulation of foreign-held dollar assets, and the loss of gold by the United States, as well as by the rise in the price of gold on the London gold market in October 1960. In contrast, he noted:

> Our Soviet ruble is today the only currency in the world whose gold content has increased compared with the gold standard period when banknotes were exchanged freely for gold.
>
> The setting of the ruble's gold content at a level higher than the gold content of the dollar is a reflection of the major economic victories scored by our people under the leadership of the Leninist Central Committee of our Communist Party.
>
> Raising the ruble's gold content and establishing its exchange rate on the basis of its purchasing power will contribute to the further growth of the ruble's international prestige and to insuring stability in international currency operations.

The depreciation of the ruble has thus been depicted as another achievement of the Soviet planned economy! This interpretation is abetted by the lapse of six months between announcement of the currency reform and the announcement of the revaluation of the ruble, and by the fact that neither in the decree announcing the revaluation nor in the lengthy accompanying statement by Garbuzov is any reference made to the currency reform (and proportional reduction in all internal prices) which will accompany the change in the exchange rate.

Garbuzov also claimed that:

> The new rate of exchange of the ruble compared with the dollar conforms to the real relationship of the purchasing power of the currencies.
>
> The new exchange rate of the ruble will make it possible to compare world prices with wholesale prices in the USSR, since, at this exchange rate, average world market prices will for the most part be brought to the level of average wholesale prices in the USSR. This will make it possible to estimate more correctly the relative profitability of the export and import of individual commodities and the profitability of the USSR's foreign trade as a whole, and its trade with individual countries.

It is indeed true that the depreciation of the ruble will bring Soviet internal prices into closer correspondence with world market prices, converted into rubles at the new rate. This may be illustrated by comparing the official ex-

change rate with the relative purchasing power of the ruble and the dollar in regard to national product, before and after the 1961 revaluation. A rough calculation of this sort can be made with ruble-dollar ratios calculated for a study of Soviet and U. S. national product. However, one should keep in mind that the relative purchasing power of the ruble and dollar would, of course, be different for different "baskets" of goods and services than those in national product, e.g., those entering foreign trade.

Table 1 shows ruble-dollar ratios for national product and its principal components for 1955.[8] For the purpose of the present analysis, these ratios may be considered sufficiently representative of the corresponding ruble-dollar ratios for 1960, inasmuch as the changes in Soviet and U. S. prices, and in

TABLE 1—RUBLE-DOLLAR RATIOS FOR GROSS NATIONAL PRODUCT AND ITS PRINCIPAL COMPONENTS

	1955 Ruble-Dollar Ratios[a]		Ruble-Dollar Ratios Resulting from Currency Reform[b]	
	Soviet Weights	*U. S. Weights*	*Soviet Weights*	*U. S. Weights*
Consumption	8	15	0.8	1.5
Investment	5	7	0.5	0.7
Defense	4	5	0.4	0.5
Government administration	2	2	0.2	0.2
Gross National Product	6	12	0.6	1.2

[a] From [2, pp. 385–86], where sources and methods are explained.
[b] One-tenth of the corresponding 1955 ratio.

the structures of the respective national products, from 1955 to 1960 were relatively modest.[9] These ratios show that the official exchange rate of 4 rubles per dollar overvalued the ruble substantially for the consumption component and to a lesser extent also for the investment component, as well as for national product as a whole. The reduction of Soviet prices in 1961 to one-tenth their 1960 level will reduce the ruble-dollar ratios correspondingly, as shown in the table. The new exchange rate of 90 kopecks per dollar is much more consistent with the resulting ratios than was the relationship between the previous official exchange rate of 4 rubles per dollar and the ratios shown in the first two columns of the table. The degree of correspondence

[8] Similar calculations and comparisons are made by Soviet government economists, although their results are not disclosed in the published Soviet literature [4, p. 109].

[9] For example, if the ruble-dollar ratios for the consumption end-use are adjusted for the increase from 1955 to 1959 in the corresponding Soviet prices of about 1 per cent [15, p. 770] [16, p. 239] and in the corresponding U.S. prices of about 9 per cent [3], the Soviet-weighted ratio would be modified to 7, instead of 8, rubles per dollar and the U.S.-weighted ratio to 14, instead of 15, rubles per dollar. Soviet data to make similar adjustments for the ratios for the other end-uses are not available, but it is to be expected that these ratios would not be significantly modified, inasmuch as Soviet wholesale prices have not been substantially changed since 1955, while U.S. wholesale prices increased about 8 per cent from 1955 to 1959 [3]. For a discussion of the importance of considering the results with Soviet and U.S. weights separately, rather than taking a geometric average of the two sets of results, see [1].

between the new exchange rate and the second set of ruble-dollar ratios compares favorably with similar relationships calculated for eight Western European countries by Gilbert [5, p. 40]. In this respect, the new exchange rate for the ruble is more "realistic" than the old: it does more nearly "conform to the real relationship of the purchasing power of the currencies," at least in purchasing national product.

While the new rate will be more satisfactory than the old rate for national product comparisons, it is by no means evident that it will also make it easier for the Soviets to "estimate more correctly the relative profitability" of Soviet foreign trade. The obstacle to a more accurate estimate of the (strictly economic) benefits to the Soviet economy from foreign trade has not been the overvalued exchange rate, which could easily be corrected for in planning the volume and composition of Soviet foreign trade, but rather the irrational Soviet internal relative price structure, which does not properly reflect relative scarcities [6]. Therefore, unless the revaluation of the ruble is also accompanied or followed by a drastic revision of the wholesale price *structure* (as distinct from a simple reduction in all prices to one-tenth their former level, with no change in relative prices), the revaluation will make no contribution to solving the problem of planning foreign trade on a sound economic basis. It remains to be seen whether the revision of wholesale prices to be undertaken in 1961-62 [10] will produce a significantly more rational internal price structure.

To summarize, the revaluation of the ruble, taken in conjunction with the concurrent currency and price reform, constitutes a depreciation of the ruble, except in so far as a limited group of invisibles transactions is concerned, for which the rate will be appreciated. However, the revaluation will not affect either the domestic price structure or the volume and composition of foreign trade. The depreciation puts the ruble at a rate which is more "realistic" in terms of comparative purchasing power with the dollar than was the old rate. Although this may facilitate some international value comparisons, such as those of the size of national product, it will not make possible more rational (economic) decisions regarding foreign trade, so long as the irrationality of the Soviet internal wholesale price structure continues.

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Some Comments on "Growth"

Output may "grow" either because inputs increase or because productivity (output divided by input) increases. The two kinds of growth are very different.

The growth of output has been slower in recent years. Between 1950 and 1955 the GNP in constant prices increased 23.5 per cent, or an average of 4.7 per cent a year. Between 1955 and 1959 the increase was 9 per cent, or an average of 2.25 per cent a year. It is quite tempting for those of us who have reservations regarding Federal Reserve policy to use the figures just cited as "proving" that tight money policy has been "stifling growth." Perhaps. But unemployment as a percentage of the civilian labor force was 5.5 per cent in 1959 and 4.4 per cent in 1955. Conceivably Federal Reserve policy may have slowed down the increase in inputs and therefore been responsible for the 1.1 per cent net increase in unemployment. But, if the increase had been eliminated so that the same percentage of the labor force had been utilized in 1959 as in 1955, the increase in the GNP over the period would hardly have been more than 10.2 per cent instead of 9 per cent, or an average of 2.55 per cent rather than 2.25 per cent a year.

What happened during the period is that the rate of increase in productivity declined significantly. If we take what appears to be the broadest possible

measure—constant dollar GNP per employed civilian worker—the increase from 1950 to 1955 was 17.6 per cent, or 3.5 per cent a year, but in the last four years the rise was only 4.6 per cent, or 1.15 per cent a year. If we allow for some reduction of hours, as derived from estimates by the Bureau of Labor Statistics, the increase is raised to 19.3 per cent, or 3.9 per cent a year, in the first period, and 6.5 per cent, or 1.6 per cent a year, in the second period. Both estimates show so drastic a decline that they almost certainly overestimate, due to statistical difficulties, what has occurred; but that productivity has declined significantly seems clear.[1]

Thus to argue that Federal Reserve policy has "stifled growth" is in essence to argue that tight money has been responsible for the decline in the rate of increase in productivity, a contention that may be a little difficult to establish. Further, if we want to resume "normal" growth, what we primarily need is a program to step up the rate of increase in productivity, not one to bring about an increase in inputs. Again, the labor movement often demands rapid growth to absorb unemployment without specifying what the word means; but, if rapid growth were caused by a rapid increase in productivity, labor's demand could be satisfied and unemployment might at the same time increase appreciably.

Because capital-output ratios have been relatively fixed in the past, it is often argued that we cannot "afford" to "grow" at, say, 5 per cent because "burdensome" capital formation of, say, 15 per cent would be required. But obviously, as many have pointed out, $3 of capital formation does not guarantee a $1 increase in output; the ratio has been relatively constant because the rate of increase in productivity has been relatively constant. We know little about the factors which determine the rate of increase in productivity, but it seems likely that research in all its aspects has been of major—perhaps even of overwhelming—importance. Between 1945 and the present time research expenditures increased from 1 to 3 per cent of the national income.[2] But rather more than half of all present research is the result of spending by the Defense Department and the Atomic Energy Commission primarily for the improvement of military hardware, and almost half of all research is done by two industries—aircraft and electrical equipment. Thus the civilian sector is responsible for less than half of all research, of which hardly less than two-

[1] Government purchases of goods and services increased from 14.2 per cent of GNP in 1950 to 18.6 per cent in 1955, with practically no change thereafter to 1959; it is possible that a shift such as this causes an exaggeration of the increase in constant dollar GNP—and therefore in the rate of increase in productivity during the first period. The Bureau of Labor Statistics, in any event, reports a very much smaller decline in the rate of increase of productivity in the private sector. It is possible that the increase in services as a percentage of personal consumption expenditures from 34.8 per cent in 1950 to 35.5 per cent in 1955 and 37.8 per cent in 1959 helps to explain the slower rate of increase in the second period. But all measures—including that of John W. Kendrick at the National Bureau of Economic Research—show some decline; and, if government purchases are included—as they should be, in my judgment, if we are to obtain a proper over-all measure of economic performance—it seems likely that the decline will turn out to be appreciable even when the statistical difficulties are solved.

[2] D. M. Keezer, "The Outlook for Expenditures on Research and Development During the Next Decade," *Proc., Am. Econ. Rev.*, May 1960, *50*, 355 ff.

thirds is for product development. (The Styling Laboratory is an important part of the Technical Center of a major automobile producer.) Thus no more than ½ of 1 per cent of the national income is spent on both civilian applied research and total fundamental research, including the fundamental research financed by the military.

The difference between the private and the social returns from research is, in my opinion, immense. As additions to knowledge cannot be patented, there is little profit incentive for firms to undertake basic research; to benefit they must either try to keep the results secret or make them available to all competitors without charge. Moreover, even in the case of applied research, the patent system as presently operated permits private firms to receive, in my judgment, only a relatively small part of the social returns from such research. In fact, if research were carried on as a *separate activity*, it seems to me quite unlikely that it would be able to sell its output at a price sufficient to cover its cost of production. Very few are in the business of inventing; most research is carried on by or for companies with products to sell. Admittedly firms with products to sell are sometimes able to obtain additional returns if they can monopolize the superior products or processes that applied research is likely to provide. But such gains are almost certainly temporary, and the whole relationship between research spending and private returns highly erratic. Under the circumstances it seems clear that spending on basic and civilian applied research is well below the socially desirable level.

But suppose in some way or other we managed to triple our spending on both basic and civilian applied research by devoting to such research an additional 1 per cent of income. Such a sharply expanded research effort might appreciably reduce the capital-output ratio. Specifically, it is possible that an additional 1 per cent of income devoted to research might permit a 5 per cent growth in output with no more than 9 per cent of current production devoted to capital formation, reducing the total "burden" of rapid growth to 10 per cent of current production rather than the 15 per cent that might be needed if the capital-output ratio were to remain unchanged.

The fact that such a result is "possible" does not make it probable—much less certain; but surely the possibility deserves careful investigation. It is suggestive that the rate of increase in productivity in agriculture—the one area where government has concerned itself systematically with the performance of an industry—has been more rapid since the second world war than in any other area of the economy except electric utilities. Yet even as careful a student as Theodore W. Schultz has concluded that "the resources committed annually [to agricultural research] would have to increase very substantially before the rate of return from this stream of inputs would not exceed that obtained in production activities generally."[8] If this is true of an area where the government *has* concerned itself with performance, what are likely to be the returns in other areas?

One alternative—which apparently is going to be adopted—is to liberalize depreciation allowances. Undoubtedly any increase in the rate of capital formation improves the average quality of the capital stock and, therefore, has

[8] T. W. Schultz, *The Economic Organization of Agriculture*, New York 1953, p. 113.

favorable repercussions on productivity. But few if any industries are today suffering from any shortage of capacity, so that the response to liberalization may well be small; certainly it is hard to believe that the expansion of capacity can be as rapid in the near future as it has been in the recent past.[8] In any event—to sum up these comments in a single sentence—what we urgently need, as I see it, is not *more* capital of basically the same sort as we now have but significantly *better* capital embodying the improved technology that can be expected to result from an expanded program of research and development.

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[8] I would give the substantial increase in capacity—and therefore in competition—major credit for the decline in corporate profits as a percentage of the national income from 14.7 per cent in 1950 to 11.6 per cent in 1959.

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Hospitalization Insurance and Hospital Utilization

In Dallas, Texas, in 1929, Justin Ford Kimball, executive vice-president of Baylor University, originated a plan whereby Dallas school teachers paid $6 a year to the University Hospital and were extended hospital care without further charge [1, p. 5]. From this beginning grew Blue Cross plans, pioneers in the health insurance field. Commercial insurance companies became important sellers of health insurance after 1941. More recently, independent health plans such as those sponsored by trade unions and management have emerged. At least 1,150 different organizations provide voluntary health insurance today [2, p. 47].

At the end of 1957, about 121 million persons owned some form of hospitalization insurance. Ten years earlier the number was only 53 million [2, p. 41]. The growth and extention of medical care insurance shows no sign of slowing. Pressure upon the U.S. Congress to expand coverage among older persons is mounting.

What consequences may be expected from continuation of the trend toward more comprehensive medical care coverage? The answer is surely as complex as it is important; in the present paper we hope to shed some light on the matter and to suggest directions for further research. Specifically we report the results of a statistical study of the impact of extended hospitalization-insurance protection on the utilization of hospital services. Our objective has been to consider the questions of how and to what extent people are likely to react to an increase in the availability of prepaid hospital services. Implications of such findings for the social costs of expanded health care, whether under governmental or private auspices, should be clear.

I. *The Method and Setting*

Comparison has been made of the hospitalization experience of two large groups of persons before and after the extension of additional service to one of them. The two groups studied each consisted of between 700 and 900 subscribers (plus their dependents) to Blue Cross Hospital Care plans. The groups consisted of employees of firms in the same (service) industry; the firms were of similar size, and were located only a short distance apart in the

St. Louis area. Being closely competitive, the firms would be expected to have reasonably comparable personnel policies and wage structures, and employees with similar education. (It was not possible to obtain information about these variables.) Since both groups were in the St. Louis area, we would expect them to be similarly affected by environmental factors, such as local health conditions, the quality of hospital facilities and the relative availability of hospital beds.

Some differences existed, however, in the age and sex distributions of the groups, as indicated in Table 1:

TABLE 1—DISTRIBUTION OF BLUE CROSS SUBSCRIBERS, GROUPS P AND S, BY AGE AND BY SEX, 1957 AND 1958

Age and Sex	Percentage of Total Subscribers			
	Group P		Group S	
	1957	1958	1957	1958
Age:				
16–25	21.7	21.3	30.6	29.6
26–35	12.7	12.2	10.8	11.5
36–45	18.1	17.4	13.7	12.1
46–55	26.5	25.0	27.3	27.0
56–65	19.0	21.7	17.6	19.5
66–75	1.6	1.9	—	0.3
76–85	0.3	0.5	—	—
Sex:				
Male	47.8	49.1	51.2	50.5
Female	52.2	51.9	48.8	49.5

Referring to the group retaining the "standard" Blue Cross coverage as "S", and the group adopting the more liberal "preferred' Blue Cross coverage as "P," we may point out further that the mean age of female subscribers of group P was six years greater than the mean age of female subscribers of group S. In 1957 the mean age of group P females was 38.2 years, compared with 32.2 for group S females. In 1958 the figures were 39.3 and 33.3. Since the differences in age and sex structure were relatively unchanged during the two-year period investigated, they would not appear to exert any substantial differential impact on the comparative experience of the groups before and after the adoption of preferred coverage by one group.

During 1957, both groups had Blue Cross coverage paying standard benefits. In addition to Blue Cross coverage, group P had Blue Shield medical coverage, while group S had similar coverage, but with a commercial insurance carrier. As of February 5, 1958, one group chose to obtain the more comprehensive Preferred Blue Cross coverage. The decision was made on an individual basis, with at least 75 per cent of the eligible employees being included.[1]

[1] Costs for the standard coverage were $6.00 per month for a family membership, and $3.00 for an individual. Costs for the preferred coverage were $7.90 and $3.30. The fact that one group chose the preferred coverage with its increased premium while the other

Differences between the standard and the preferred coverages pertained to ancillary services and private room accommodations. Specifically, the most significant differences were the following: The maximum allowance for private room accommodation was $10 per day under the standard plan and $12 under the preferred; laboratory services of only a routine character were covered under the standard plan, while there was no limit to coverage under the preferred; and under the standard plan, there was no allowance for diagnostic tests, anesthesia services, and physical and shock therapy, whereas there was no limit to the allowance for these services under the preferred plan. In general, other benefits for both preferred and standard plans were the same.

Because the study was made for the first year of coverage under the preferred Blue Cross contract, the possibility exists that the members of group P might not have become fully adjusted to the additional coverage and, therefore, might not have utilized it to the extent that they would in subsequent years. But this does not seem likely, for at least two reasons: First, employees were individually contacted by Blue Cross personnel; advantages of the plan were presented and an opportunity to ask questions was provided. Second, since the preferred coverage involved an increased premium, it is to be expected that employees would become aware of increased coverage provided.

All data were obtained from records of paid hospital cases of the St. Louis Blue Cross Organization, for the years February 5, 1957 through February 4, 1958, and February 5, 1958 through February 4, 1959. Hereafter these periods will be referred to as 1957 and 1958, respectively. The analysis involves comparisons of (1) utilization rates (the number of admissions per 1,000 subscribers), and (2) usage of certain ancillary services and private rooms by the two groups.

II. *Utilization Rates*

Under the preferred coverage, ancillary service benefits were greatly expanded, but only on an in-patient basis. Therefore, one hypothesis studied was that the hospital utilization rate would rise for group P as more of its members entered the hospital in order to avail themselves of the ancillary-service benefits.

Before analyzing the experience of the two groups, we need to recognize a factor which tends to increase the utilization rate in group P during the first year of the extended coverage, but which would not be present in subsequent years. In order to encourage employees to subscribe and bring the membership up to the minimum of 75 per cent of employees required to qualify for preferred coverage, waivers were granted new subscribers for all prior conditions, including pregnancy. Thus persons with pre-existing medical conditions,

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did not perhaps reflected a difference in attitude or in anticipated use of services. Since it is precisely these matters which we seek to investigate, no problem is posed by the different decisions of the groups. So long as hospitalization (or any other) insurance is available on a voluntary basis, we may expect "adverse" selection; i.e., those taking the broader coverage will often be the ones most likely to make use of the service either because they are more illness-prone or because they are more anxious to enter a hospital than others in a similar state of health.

who could not otherwise obtain coverage for these conditions, were permitted to obtain coverage by subscribing to the preferred plan. During 1958 there were 13 admissions attributable to such cases. Since we are interested in the longer-run incentive effects of extended coverage on use of hospital facilities, these cases have been excluded in our analysis of utilization rates.

In 1957, utilization rates were 224 per 1,000 subscribers for the group which later adopted preferred coverage, and 228 per 1,000 for the group which maintained its standard coverage. This difference was not significant at the 5 per cent level. (This is the level of significance which will be used throughout.) In 1958, utilization rates fell to 193 for group P and 182 for group S—a highly significant difference. Also the difference in percentage decreases in utilization rates for the two groups—13.8 per cent versus 20.2 per cent—was statistically significant.

What factors may be said to have led to the relative maintenance of the hospital utilization rate for group P? Analysis of admissions data, shown in Table 2, disclosed that the relatively poor experience of group P was attributable almost entirely to the following:

1. Increased utilization among subscribers rather than dependents. In fact, the dependents of group P members actually had a relatively more favorable experience than the group S dependents.

Table 2—Percentage Changes in Hospital Utilization Rates, Groups P and S, by Subscriber-Dependent Status, Sex, Age, and Diagnosis, 1957–1958

Status, Sex, Age, Diagnosis	Percentage Change in Utilization Rates, 1957–1958	
	Group P	Group S
Status:		
Subscribers	− 3.1	−22.9
Dependents	− 27.2	−17.5
Sex:		
Male	− 22.9	−21.3
Female	− 13.4	−19.4
Excluding deliveries	− 14.2	−26.3
Age:		
15 years and under	− 47.4	−37.9
16–35	− 29.5	−28.6
36–55	− 8.3	− 4.4
56–75	+ 19.8	−17.1
Diagnosis:		
1. Diseases of respiratory system	− 51.9	−55.9
2. Diseases of digestive system	+ 24.2	−18.5
3. Diseases of genito-urinary system	− 8.3	−25.6
4. Accidents, poisonings and violence	+137.5	+40.0
5. Diseases of circulatory system	− 29.4	−27.3
6. Deliveries and complications of pregnancy, childbirth and the puerperium	− 10.2	− 2.2

2. Increased utilization among females rather than males. This was true whether or not admissions for deliveries were included.

3. Increased utilization among persons 56 years of age and over. Among persons over 55, group P had an increase in utilization rate in the first year of its preferred coverage, while group S had a decrease. In no other age group was the group P experience unfavorable relative to that of group S. The evidence, though not complete, indicates that the increased utilization among females, noted above, occurred among older (over 55) females in particular.

4. Increased utilization for persons with diseases of the respiratory, digestive, and genito-urinary systems; and accidents. For all of these classes of diagnoses (1-4 in the lower portion of Table 2), the percentage change in the group P utilization rate in 1958 was significantly different from the percentage change for group S, and each of the differences was in the direction of greater increase or smaller decrease in group P utilization. These four diagnoses accounted for some 50 per cent of the hospital admissions.

For diseases of the circulatory system (diagnosis 5) there was no significant difference between the percentage changes in utilization rates for the two groups. For deliveries, etc. (diagnosis 6), there was actually a significantly greater percentage drop in the group P utilization rate, though this is presumably a result of the greater mean age of women in group P.

Thus, the hypothesized increase in group P hospital utilization rates relative to group S has been supported, though it may be food for thought that the relative increase occurred mainly in the case of older females. In so far as women are secondary, rather than primary family-income earners, the opportunity cost of their hospitalization may be less than for their husbands. This, in addition to medical conditions, may help to explain the comparatively adverse experience among older females of group P.

III. *Use of Hospital Ancillary Services*

The new provisions of the preferred program included liberalization of coverage of ancillary services expenses and broadened benefits for private-room accommodations. The hypothesis investigated now is that expenses incurred for ancillary services, and the use of private rooms, would increase for group P relative to group S. Since the marginal cost of these additional services to the patient falls (to zero for ancillary services, though not for private rooms), we might expect increased use.

The increase in total ancillary service costs per admission for group S (see Table 3), was not significant, but the increase for group P was significant.

When we investigate experience of the two groups with regard to usage of private rooms, we find that the relative frequency of use of private rooms declined for both groups between 1957 and 1958. For group P the decline was from 19 to 17 per cent of total admissions; for group S the decline was from 21 to 17 per cent of total admissions. Though the drop for group P was proportionately smaller than for group S, the difference was not significant.

The hospital charge for a private room was close to $18 per day. Apparently the differential in cost to the patient between the Blue Cross allowance ($12 per day) and the hospital charge for a private room was sufficiently sub-

TABLE 3—MEAN COST PER HOSPITAL ADMISSION FOR CERTAIN ANCILLARY SERVICES, GROUPS P AND S, 1957 AND 1958

Service	Cost Per Admission			
	Group P		Group S	
	1957	1958	1957	1958
Diagnostic X-ray	$16.25	$19.97	$14.91	$14.00
Clinical laboratory	8.20	12.20	7.46	6.52
Other[a]	15.28	20.40	13.42	16.82
Total	$39.73	$52.57	$35.79	$37.34

[a] Includes routine laboratory, basal metabolism, physical therapy, X-ray therapy, radiation therapy, and electrocardiograms.

stantial so that the extra $2 allowed group P members did not significantly influence their private-room use.

IV. *Summary and Conclusion*

As might be anticipated, the group electing the broader, preferred Blue Cross coverage, did increase their utilization of hospitals relative to that of the group retaining standard coverage. The increased availability of ancillary service benefits apparently led to relatively more frequent use of hospitals by group P members. The relative increase in the group P utilization rate, however, was not found among all subsets of group P. Rather, the increase was concentrated among females over 55 years of age.

Among those group P members entering hospitals we discovered a significant increase (relative to group S) in their use of ancillary services, which were covered in full. Yet, no significant increase appeared in private-room use, benefits for which were also increased to group P members (though only partial coverage was provided).

One hypothesis which seems to merit additional investigation is that the extension of hospital care services—whether through private or public programs—may have significantly different impacts among the sexes and among persons of different ages. If this is the case, the financial implications of extended medical care may be related in an important way to the present and future age and sex structure of the populations involved.

Second, it would be fruitful to study the possible existence of benefit "thresholds." By this we mean that, up to some point, increased benefit allowances may only shift part of the financial burden of hospitalization from the patient to the insuror without leading to increased total costs. To the extent that some out-of-pocket payment by the patient is required (as in the private-room case even under the preferred Blue Cross plan), a reduction of the patient charge—but not to zero—may have little or no incentive effect on utilization. Thus, the relative frequency of use of private room accommodations, for example, may be quite inelastic with respect to patient charge over a significant range, but may become more elastic at charges very close to zero.

This is more likely to be the case where "good," lower-cost substitutes are available; semiprivate rooms under the Blue Cross Plan are an example, because no patient charge was made for these accommodations, while there was a charge for the private room.

It is commonplace to assume that if medical-care coverage is broadened, total national costs will not merely be shifted among people, but will always be increased. Whether this is correct, and equally so for all age, sex and other groups of the population and for all magnitudes of extended coverage, is by no means clear. The present study suggests that the assumption is open to question.

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* The authors are, respectively, assistant professor of economics at Washington University, St. Louis, and 1st Lt., U.S. Army. We wish to acknowledge helpful comments by Werner Z. Hirsch and Herbert Fraser on an earlier draft. We are also indebted to Group Hospital Service, Inc. (Blue Cross), St. Louis, and its president, Oscar W. Rexford, for their cooperation.

The Burden of the Public Debt: Comment

In a recent note Messrs. Bowen, Davis, and Kopf present a case for the classic view that deficit financing of public expenditures places a burden on future generations, as compared with pay-as-you-go financing [1]. The purpose of this comment is to suggest that they are right for reasons that are, if not wrong, at least needlessly roundabout and largely irrelevant, and that former President Eisenhower was right, if at all, only under circumstances that are still far from being realized.

The authors of the note base their discussion on what would, in non-emergency times, be a rather unlikely reaction of individuals to the change in fiscal policy in question, namely the financing of bond purchases entirely by curtailing consumption, and trace the results through generations of individuals in a way that tends to obscure some of the fundamental repercussions on such matters as investment and the marginal productivity of labor. Their analysis may have some validity in a borrowing and rationing situation such as occurred during the second world war, but it seems inapplicable to periods where individuals are in fact free to expand their total consumption. The Eisenhower statement is less specific, but while its implications could be supported in a context where full employment was assured, this was so far from being the case in early 1960 that it must be considered at least ill-timed. The analysis that follows is equally irrelevant to the current situation, in that it attempts to trace the consequences of debt finance under full-

employment conditions. However, it is based on assumptions about consumer behavior that might be more appropriate to an era characterized by a vigorous full-employment policy but without widespread direct controls such as rationing. Unlike Bowen, *et al.*, no attempt is here made to separate out the generations in terms of age groups; for simplicity the population at a given epoch is considered as a whole, except as it may be appropriate to consider a subdivision into recipients of labor income and property income respectively.

The crux of the problem of the relative burden of debt finance and tax finance is that monetary and fiscal policy together provide two major degrees of freedom for over-all public policy by means of which control can be exercised, within limits, over two basic parameters of the economy: the aggregate level of activity and consequent output, on the one hand, and the way in which this aggregate output is apportioned between current consumption and capital formation, on the other. A firm commitment to the maintenance of a given level of activity and employment will use up one of these degrees of freedom; but within this commitment there remains one degree of freedom in the choice of suitable combinations of fiscal policy and monetary policy. If a combination of monetary and fiscal policy is chosen which offsets the greater inflationary effects of lower taxes and more borrowing with restrictive monetary policy and higher interest rates, this will lead to more consumption, less investment, and thus to a retardation of economic growth and a reduction in the heritage of accumulated resources to be handed on to future generations.

Where Bowen, Davis, and Kopf go wrong is in asserting that "resources consumed by a debt-financed public project must entail a contemporaneous reduction in private consumption" [1, p. 703]. If we assume full employment to be maintained whether the project is debt-financed, tax-financed, or not undertaken (and without this assumption the whole argument collapses), and if we assume a "public debt illusion" under which individuals pay no attention to their share in the liability represented by the public debt in determining how much of their income they will spend, we can expect consumer demand to be higher when the project is debt-financed than when it is tax-financed; if inflationary pressures are to be avoided, aggregate demand must be kept within the capacity of the economy by curtailing private investment demand, the increased demand for borrowed funds represented by the debt financing must be allowed to tighten the money market, and if necessary supplementary monetary measures must be adopted so as to lower liquidity, drive interest rates up and generally increase the difficulties of financing to the point where private investment is curtailed sufficiently to remove the inflationary pressure. Only if savings were highly interest-elastic and investment highly inelastic, or if the project financed were specifically such as to substitute directly for consumption expenditure could it be assumed that the resources used would be derived from a reduction in private consumption.

The shifting of the burden to the future that is produced by debt financing is then essentially the shifting of resources out of private investment and into consumption that is induced by the change in method of financing. The relationship, however, may not be dollar for dollar. On the one hand, the

marginal propensity to consume may be less than 1, so that a reduction in taxes of $100 might cause an increase of spending by only $80. Further, the tightening of the money market may have repercussions on consumption, so that the actual amount of added consumption and added burden on the future is only $70. At this point, however, some caution is needed, for the main effects of monetary stringency are likely to be on expenditure for housing and durables, and a precise evaluation would have to consider changes in the residual value of consumer durables as part of the change in the heritage carried into the future, so that only repercussions on net current consumption would be relevant.

Actually, this analysis can show an element of future burden even where additional current public expenditures are financed out of current taxes. If we can assume that the added current public expenditures are not such as to change the consumption function expressed in terms of disposable income, then the financing of $100 of added outlays by $100 of taxes will reduce consumption by only say $80, and if $100 of resources are to be freed for the public purpose, an additional $20 will have to come out of private investment, whether through a tightening of the money market or, if the public expenditure is in some way less of a stimulus to private investment than is the consumer demand it replaces, through the acceleration effect of the decline in consumer demand. If the public expenditure is indeed of only transitory benefit, the heritage left for the future will then be decreased by $20. If the public expenditure of $100 is to be financed in such a way as not to diminish the heritage left for the future, it would be necessary to levy taxes of $125, so that individual consumption would be reduced by $100, and the $25 surplus would serve to replace in the capital market the vanished individual savings of $25.

In this analysis it makes no difference whether the heritage is regarded as being left to future generations considered as different individuals or merely to the later years of the same persons. There is, however, one further aspect of the future burden that is important. In the Bowen-Davis-Kopf representation, where the resources required by the project come from consumption, the productive capacity of the economy is unimpaired, and indeed if the interest payments are financed by further borrowing, the final apportionment of the "burden" is deferred until this accumulated debt is retired; if the economy is growing at a rate as great as the rate of interest, there is no essential reason why this cannot be put off indefinitely, so that no identifiable group is harmed, on balance, and the "burden" vanishes from sight. If there is any burden here, it lies in the impairment of the capacity of the future generations to pull this stunt themselves! This paradoxical situation may perhaps be taken as indication that a long-term interest rate lower than the growth rate represents an unstable situation. In any case, there exists an unavoidable "real" burden on future generations whenever the more tangible resources available to the future generations are impaired. If in the face of such an impairment of resources an attempt is made to avoid the burden on any particular generation by maintaining consumption at the level it would otherwise have reached, further impairment of capital will take place, with an ultimate day of reckoning.

In an epoch where we contemplate with increasing equanimity the possibility that the national debt might be allowed to grow indefinitely, at least in absolute terms if not as a fraction of national income, some allocation of the burden is needed other than in terms of the taxpayers who finance a retirement of the debt that may never occur. If debt financing results in a lower level of investment (as compared with tax financing of the same government outlays), so that some future generation finds itself with a lower endowment of real capital, who in particular are the losers? If at this point we invoke a strictly competitive economy with a classically homogeneous production function with diminishing returns to increments of each factor as one or more of the others is held constant, we find that interest rates and the marginal productivity of capital are higher than they would otherwise have been, while wage rates are lower than they would otherwise have been by an amount sufficient not only to absorb the entire reduction in output caused by the reduction in resources, but to allow for the increased rate of return on investment. All of which can be summed up by saying that, *given full employment*, shifting the fiscal policy-monetary policy mix in the direction of debt finance tends to place a burden on the future measured by a fraction of the debt increment somewhat smaller than the marginal propensity to consume; that in the absence of debt retirement or other compensatory action the primary burden of diminished future income will be felt by future wage earners, and that there will in addition be a tendency for the income distribution to shift in favor of property incomes.

While this analysis does depend on a public-debt illusion or its equivalent, elimination of this factor eliminates the shifting to the future entirely, and with it, indeed, the effectiveness of fiscal policy as an instrument of stabilization. It is perhaps paradoxical that it is precisely in the field of local finance where the debt burden is most clearly a charge against identifiable pieces of property and the public-debt illusion should be at its weakest that the virtues of "pay-as-you-go" have been most universally applauded. Indeed it is here that the property owner in a town that floats a debt instead of raising the property tax rates can maintain a financial status equivalent to that of his peer in the neighboring debtless community by taking his tax savings and stepping up his mortgage payments. The reduced mortgage balance will offset the decline in market value produced by the overlying public debt, so that his equity will remain unchanged. Aside from the gain he may realize by reason of the margin between the interest rate on his (taxable) mortgage and that incurred on the (tax-exempt) public debt, he cannot ultimately escape his share of the burden even by dying penniless, for in the sale of his property a rational market will have capitalized the future debt service burden allocable to it. To be sure, a rigorous analysis is complicated by the fact that new construction in a debt-burdened community will be discouraged by the consideration that improvement will increase the share of the debt burden borne by the property being improved. But by and large, assuming a fully rational market, local debt finance will not shift the burden to the future, though it may, if carried too far, cause future administrations to have financial headaches, particularly if they are hemmed in by statutory limits on tax rates and debt rates.

If the unrealistic assumption is adhered to that the resources are taken entirely from current consumption, then it is fair to say that it is not the undertaking of the expenditure on the basis of debt finance that has imposed a burden on future generations, but rather the decision of the present generation as individuals not to bequeath the bonds thus acquired to their heirs in the subsequent generation. If individuals of this generation have objections to imposing a burden on the future, it will always be open to them to avoid this result by increasing the amount they individually bequeath to the future generation by an amount sufficient to pay off the debt. Moreover, while this is always possible (and would not even be frustrated by the imposition of death duties, since the added revenues thus produced would also be available to retire part of the debt), the reverse option, *i.e.*, that of taking individual action, where the project is tax-financed, which would impose a burden on the future generations, is less completely available: it is often difficult to arrange to leave a negative estate when the estate was originally zero or very small. On this basis one could even argue that debt financing is the preferable policy in the abstract, in that it leaves a wider range for individual choice: if policy B gives individuals the freedom to choose actions which will bring about all of the results that are possible under policy A, whereas under policy A there are no available actions which would reproduce some of the results available under policy B, then B is necessarily to be preferred. The need to deny this proposition if pay-as-you-go financing is to be advocated may incidentally compel recognition that the "conservative" point of view in this case necessarily implies the acceptance of some kind of social as distinct from individualistic values.

Thus far the discussion has been largely in terms of relative interests of groups of individuals, in the individualistic tradition of classical economics. The more popular concern, nowadays, is with the promotion of economic growth. It is apparent from what has been said above that to the extent that growth is a result of capital formation, policies involving excessively large amounts of debt financing are inimical to growth. Indeed, if short-run stability were not in question, the maximum rate of growth compatible with a level price trend and a given pattern of government outlays would be obtained by having a maximum degree of monetary ease, the lowest possible interest rates, and a level of taxation sufficiently high to offset the resulting inflationary pressure, probably but not necessarily involving a substantial budgetary surplus.

Maintaining maximum growth under these circumstances involves abandoning monetary policy as a means of checking downward fluctuations, and having to rely solely on more cumbersome and slower acting tax or expenditure changes as a means of correcting deflationary developments. Moreover, in an open economy, the "growth" may take the form of foreign investment and the accumulation of claims to income from abroad, a development that may on the one hand involve risk of default or expropriation and on the other may forfeit whatever local benefits may derive from external economies.

If a more rapid rate of growth than this is desired, or if it is felt essential to maintain a larger reserve of potential monetary stimulus, this can only come

about either through a larger scale of direct government investment, through shifts in tax policy from taxes on property and on corporate income and the like to taxes on personal income and consumption, through outright subsidy to private investment, or through the adoption of policies which will make inflation of the price level (not necessarily uncontrolled or accelerating) the normal expectation of investors generally [2]. In this last case we would have the paradox that while a large public debt is generally considered to be inflationary, deliberate inflation might be the means whereby full employment would be made compatible with a reduction in the real debt and an enhancement of the heritage being bequeathed to the future.

In any case the essential precondition is *given full employment*. The analysis is relevant only where it is assumed that some given level of employment is to be maintained. If variations in fiscal policy are undertaken without the proportionately vigorous correlative monetary measures needed to stabilize employment and prices, then a surplus, far from being "a reduction in our children's inherited mortgage" [3] can easily give rise to increased unemployment and a multiplied and fruitless burden on both the present and the future. In the context of January 7, 1960, the multiplied burden would seem to have been a more likely outcome than the reduction of the mortgage, and indeed we may yet be a shockingly long way from being able to count on that degree of coordination and vigor in the application of monetary and fiscal policy that would make the classical analysis of the debt burden once again relevant and the Eisenhower attitude regarding the virtues of a surplus an appropriate one.

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The Burden of the Public Debt: Comment

In "The Public Debt: A Burden on Future Generations?,"[1] Messrs. Bowen, Davis and Kopf have failed to prove President Eisenhower right and the majority of professional economists wrong; but they have given us new insight into the problem by introducing, perhaps not quite deliberately, a useful distinction between social cost in the sense of resources diverted and subjective burden as experienced by individual citizens. We remain unconvinced that the social cost of a public project can, without an external debt, be shifted from the time when it is incurred; but the authors seem right in saying that its burden can be so shifted if by burden is meant what individuals consider a

[1] This *Review*, Sept. 1960, *50*, 701-6.

burden: the balance of private costs and private benefits, corrected for changes in disposable income occasioned by the public debt.[2]

If carried out at a time of full employment, a $1 billion public project, however financed, involves the *cost* of $1 billion worth of resources diverted from consumption and/or private capital formation; but it imposes no *burden* on the community if people voluntarily give up their command over these resources in exchange for government bonds. By buying the bonds, the public reveals its preference for the bonds over current consumption or private securities and incurs no more of a burden than if it had bought Ford stock or Ford automobiles instead.[3] A burden is imposed on the public only if *and at the time when* the promise written into the bonds is either formally broken or broken in spirit. This happens, for example, when the public project financed by the bonds fails to raise the gross national product and the government pays interest on the bonds out of additional taxation. In such a case, the interest payments fail to add to disposable income and thus fool the public. This constitutes, in a sense, a breaking of the government's original promise, since the public sacrificed consumption and/or private capital formation in exchange for the promise of additional income (the interest on the bonds) which it is not getting. Bowen *et al.* are right in arguing that the burden so imposed is a genuine burden and that it is distributed over the entire lifetime of the bonds —as well as over the lifetime of all future bonds that may be issued for the sake of redeeming the original ones.[4]

I part company with the authors when they speak of yet another burden imposed by and at the time of the redemption of the bonds. They argue that when the government repays the debt, the additional taxation reduces disposable income and hence consumption, while the replacement of the public's holdings of bonds by cash has little or no effect on its market behavior, so that on balance consumption is reduced, which, they assert, is the main burden of the debt. This would be all right, except that the authors forget about the crucial assumption of full employment they made earlier. If full employment and stable prices obtain when the debt falls due for redemption and an unenlightened government raises taxes (or lowers public spending) in order to create the budgetary surplus needed to redeem the debt, then the redemption will lower GNP and inflict a burden on society—but the assumption of full employment has thus been abandoned half-way. If on the other hand the government successfully offsets the restrictive effects of its budgetary surplus by a monetary policy designed to encourage private investment, then the resulting rise in GNP will offset the effect of higher taxes on disposable income and no burden will be imposed on the public. Or again, if all this happens at a time of inflationary pressures and the government redeems the debt as part of its price-stabilization policy, then again no additional burden is imposed; for the

[2] This correction is necessary, because individuals in their economic calculations always assume their incomes to be fixed.

[3] It might be argued, however, that the purchase of war-bonds under pressure of patriotic appeal does involve a burden; but is this more of a burden than that imposed on the buyer of a new car who acts under the influence of high-pressure salesmanship?

[4] In the language of opportunity costs, this burden is the foregone fruit of growth sacrificed.

additional taxes merely accomplish what in their absence the rise in prices would have done.

TIBOR SCITOVSKY

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The Burden of the Public Debt: Comment

The Bowen-Davis-Kopf thesis presented in "The Public Debt: A Burden on Future Generations"[1] purports to show that, under certain circumstances, the burden of a portion of the national debt can be shifted to future generations. What Bowen *et al.* have succeeded in demonstrating is that a given generation (Generation I) *could* pass the burden of a deficit-financed project to a future generation (Generation II) by selling their bonds (to Generation II) and using the proceeds to raise their level of consumption.

The possibility of this eventuality, it should be noted, existed long before the days of multibillion-dollar deficit finance and existed even in the absence of deficit finance. The older members of any economy which incorporates the principles of private property and the right of transfer are always free to sell their creditor claims. The sale may be made to other members of Generation I or to those of Generation II. If, during the lifetime of Generation I, there had been no deficit finance, they would still be free to liquidate *other* forms of creditor claims—life insurance, industrial bonds, common stock, etc. As a general rule, however, Generation I does not sell its creditor claims (or savings) to Generation II—the transfer is made by bequest.

None of this denies the possibility of the Bowen-Davis-Kopf conclusion; it merely questions the plausibility. In special cases, as where, for example, substantial damage to the productive facilities of the economy, or to some factor, during the lifetime of Generation I result in a low level of living relative to previous levels, the bondholders would be tempted to liquidate their bonds in an attempt to maintain their standard of living. In such a case, the shift of burden would, of course, be more probable.

However, even if one assumes that Generation I does sell its bonds to Generation II, it does not necessarily follow that increased consumption by Generation I after the transfer has occurred will require decreased consumption by Generation II. Conceivably, if this bond transfer takes place during a period in which substantial quantities of economic resources are idle, Generation I's attempt to increase consumption may increase aggregate income and, in turn, lead to a general increase in consumption—including consumption by Generation II. Thus, the existence of unemployed resources at the time of Generation I's increase in consumption would dissolve "the burden of the debt."

Reflection upon the years following the second world war lends the above consideration additional importance. While we describe the period as one of full employment punctuated by mild recessions, in few of the years, if any, were resources utilized to the fullest extent. Certainly, in most of these years a net increase in real GNP could have been attained had consumer demand (or investor or government demand) been greater.

[1] This *Review*, Sept. 1960, *50*, 701-6.

But if there are no idle resources, as Bowen *et al.* assume, then Generation II gets just what it bargained for—it exchanges current consumption claims for claims against future goods.

While these considerations confine the Bowen-Davis-Kopf thesis to a rare application, no challenge to the internal consistency of the thesis is intended. There is, however, a weak point in the Bowen-Davis-Kopf chain of logic: the implicit assumption that increased taxes necessarily reduce aggregate *real* consumption. They state: [1, p. 703]

> . . . suppose . . . that during the lifetime of Generation II the government decides to retire the debt by levying a general tax in excess of current government spending and using the surplus to buy up the bonds that are now held by members of Generation II. The inevitable outcome of this decision is a reduction in the lifetime consumption of Generation II. The taxpayers of Generation II forego consumption in order to retire the debt and yet the bondholders of Generation II do not experience any net lifetime increase in their claims on consumption goods since they are simply reimbursed for the consumption foregone at the time when they (Generation II) bought the bonds from Generation I. Conclusion: the burden of public project *X* rests squarely on Generation II, and not on Generation I.

If, however, taxpayers reduce consumption in order to pay additional taxes, who will consume the goods made available? Surely not the bondholders whose bonds are retired. Had they wished to consume, they would have sold the bonds on the market or not bought them in the first place. The bond retirement will force the (former) bondholders to shift their savings into other forms—insurance, equity securities, industrial bonds, etc. Bowen, *et al.*, implicitly assume that the bondholders of Generation II, having been bought out, would use the proceeds for consumption. This is a completely unwarranted assumption. The bond retirement program simply changes the form of the assets of the (former) bondholders from bonds to deposits but does not increase their income or consumption.

As the government (of Generation II's lifetime) raises the tax surplus to retire the debt, one of two possibilities may occur: (1) aggregate *real* consumption does not decline (although aggregate dollar consumption does) in which case the bond retirement must have been effected during a period of inflationary pressure; or (2) aggregate real consumption does decline, forcing GNP and investment down, and possibly initiating a chain reaction.

In the first case there has been no burden whatever on Generation II. The real standard of living remains constant and the debt has been repaid from otherwise-inflationary purchasing power. In the second case (which is actually outside of the Bowen-Davis-Kopf frame of reference because they assume full employment), there is a very real burden but it follows from poor fiscal policy rather than from the repayment of debt.

Were prices free and flexible, a surplus in taxes and a concomitant reduction in dollar consumption would lead to lower prices—leaving the taxpayer with fewer dollars for consumption but with correspondingly lower prices. In this case, too, the repayment is burdenless. In an economy with sticky, administered prices, however, a surplus cannot be accumulated through taxation

during a potentially deflationary period without serious consequences. But the consequences do not follow from the repayment of debt—rather from poorly conceived fiscal policy.

James R. Elliott
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The Burden of the Public Debt: Reply

We welcome the comments by Vickrey, Scitovsky, and Elliott. At the same time, however, we must insist that none of these comments alters our central point—that debt financing, by means of the intergeneration transfer process described in our original paper, can serve to shift the burden of government spending from present to future generations.

The main thing to be said about Vickrey's paper is that it represents a different approach to the debt burden problem. Whereas we chose to analyze the debt burden in terms of the distribution of lifetime consumption between generations, Vickrey has chosen to analyze the effects of debt finance on the future level of real income for society as a whole.

From this latter vantage point, Vickrey is certainly entitled to object to our assumption that bond purchases are financed entirely out of consumption.[1] In the real world an increase in the public debt will undoubtedly lead to a reduction in both consumption (C) and investment (I); and the secular growth in GNP will be slower the greater the reduction in I relative to C.

But this is by no means the only way in which debt financing can alter the *relative* economic position of different generations. Vickrey's line of reasoning is in no way inconsistent with the important point that, regardless of whether loan finance reduces C or I, loan finance can result in intergeneration transfers of burden. No matter what happens to C and I, Generation I (the present generation) is going to enjoy a higher level of lifetime consumption relative to the consumption of future generations if government expenditures are financed by issuance of debt instruments than if taxes are employed. Under the tax option, Generation I loses either immediate consumption or private claims against investment goods and receives no monetary asset in exchange; under the dept option, Generation I also sacrifices some combination of C and I, but in this case receives an asset (government bonds) in exchange, and thus enjoys the option of selling these bonds later in life to obtain either consumption goods or claims against private investment. Consequently, the assumption that loan finance reduces private consumption is in no way essential to the logic of our argument, and was used to highlight the otherwise unrecognized fact that even if loan finance fails to dampen private investment, the present generation can still shift at least a part of the burden of government spending to future generations.

Another way of making the same point is to note that even if the method of finance were to leave the investment-consumption mix unaltered, the loan-finance technique would make the members of Generation I better off relative

[1] There are several places in our original paper where it appears that we *assert*—rather than assume—that borrowing affects only consumption. This was unfortunate, and we apologize for any resulting confusion.

to subsequent generations than would the tax-finance method. Since the loan-finance procedure does, in fact, alter the mix against private capital formation, Vickrey's argument should be regarded as supplemental to our own. Our major quarrel with Vickrey's paper is, therefore, that it does not make clear that his position complements—rather than competes with—the position stated in our original paper.

The papers by Scitovsky and Elliott do deal directly with our intergeneration transfer argument, and we are indebted to these authors for pointing out that higher tax collections *cum* debt retirement are going to have a fiscal effect that challenges our full-employment assumption. But, Scitovsky and Elliott are wrong if they mean to suggest that we must abandon our full-employment assumption if we are to prevent the debt-retirement phase of our argument from collapsing.

We are quite prepared to assume (as we have all along) that, by some method such as flexible monetary policy, full employment and price stability are maintained continuously. Now, if a tax surplus is used to retire debt, monetary policy will have to be used to restore full employment. If this is done, Scitovsky and Elliott argue, the reduction in consumption engendered by the tax surplus will be just offset by the rise in GNP brought about by compensatory stabilization policies, and so we shall be exactly where we would have been if no debt retirement had occurred, and therefore there will be no burden on generations alive at the time the debt is retired.

There are two related fallacies in this line of reasoning. First, Scitovsky and Elliott seem to have forgotten that our definition of burden runs in terms of reduced lifetime consumption, not income foregone at a moment of time. Second, Scitovsky and Elliott neglect the asset effect of the debt retirement operation. Taking account of both these considerations, the following picture emerges: Generation II gave up consumption to Generation I when it bought the bonds from Generation I; the bonds are, of course, the means whereby Generation II hopes to recapture, later in life, the consumption given up early in life. It is true that at the point of time when these bonds are retired, thanks to the compensatory stabilization policies, there need be no drop in the private spending of Generation II. What does happen, however, is that Generation II is stripped of its bonds, and thus loses the assets that it had hoped to use later in life to recoup the consumption foregone early in life. It is by destroying these claims against future consumption that debt retirement locates the burden of the public project on generations alive at the time the debt is retired. Consumption was transferred from Generation II to Generation I when bonds were purchased by Generation II, and now debt retirement has extinguished the possibility that an equivalent amount of consumption could later be transferred from Generation III to Generation II. Hence, on balance, over its lifetime, Generation II has suffered a reduction in consumption even though aggregate real consumption for society as a whole need never have been affected by the whole operation.

We must also be careful not to make the mistake of saying that the public has been given additional money in place of bonds and so has suffered no net reduction in its assets. The money paid to bondholders is nothing but the

money given up by taxpayers. Therefore, the aggregate stock of money in the hands of the public is unchanged by the government's fiscal operation, while the public's bondholdings are reduced—hence Generation II suffers a net reduction in its assets. And, as a consequence, Generation II has fewer assets to carry into the future as a means of competing with Generation III for consumption goods produced in later years than would have been the case if debt retirement had not occurred.

Finally, it is important to note that the above logic is unaffected if compensatory monetary policy (as is likely) fills the largest part of the deflationary gap created by the tax-induced reduction in consumption, not with new consumption, but with new investment. In this case the new claims against investment goods of course represent claims against future consumption. These claims can be considered either as a substitute for the immediate loss of consumption attributable to the debt-retiring taxes or as a substitute for the former claims against future consumption (*i.e.*, the government bonds which have now been retired). But, these new claims against private investment cannot *simultaneously* be considered a substitute for both. Consequently, the creation and retirement of public debt can lead to a redistribution of lifetime real income between generations, given conditions of full employment, regardless of differential effects on consumption and investment.[3]

William G. Bowen*
Richard G. Davis
David H. Kopf

[3] Five very brief comments are in order, although these do not cover all of the points we would like to discuss if space permitted: (1) For simplicity of exposition we have assumed that all taxes levied to retire the debt fall on Generation II; if Generation III is also old enough to share in the tax payments, then Generation III will also share in the burden since their net assets (claims against future consumption) will also be reduced by the combined tax-collection and debt-retirement operation. (2) Throughout this discussion we have been considering debt retirement only in the strict sense of bonds retired through a tax surplus, and have not considered the case where the government "monetizes" the debt. This monetization might be deliberate, or it might arise if the government, faced with falling national income because of its debt-retirement policy, created (and spent) new money. (3) will not affect real incomes, but will reduce net assets and thus extinguish the claims against and Elliott argue) vitiate our argument. Here again, higher taxation *cum* debt retirement The existence of inflationary pressures at the time of debt retirement does not (as Scitovsky future consumption that Generation II received in exchange for its earlier transfer of consumption to Generation I. (4) Nor does consideration of the liability side of the public debt invalidate the conclusions stated above. Whereas government bonds are clearly an asset to the individual and influence his financial planning, individuals do not normally consider their full share of the public debt as a personal liability in calculating their own net worth. And, this is not merely an "illusion," since the taxpayer knows that his death will extinguish his responsibility for this obligation. (5) The reader who would like to see, in a somewhat different context, an arithmetical example of the way debt financing can be used to affect the relative economic positions of different generations is referred to R. A. Musgrave, *The Theory of Public Finance*, New York 1959, pp. 562-65. Musgrave also discusses Vickrey's topic (the effects of debt finance on consumption versus investment) in considerable detail.

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Measuring the Success of the Elementary Course: Comment

In the March 1960 issue of this journal there appeared an article by Simon Whitney summarizing the results of his attempt at measurement of the degree to which students in economics assimilate the material presented to them [2].

Unfortunately the main force of the Whitney analysis is a rather dismal prognosis for the principles course in particular and, in fact, for the teaching of economics generally. Specifically Whitney concludes: (1) that, on the basis of a before-after comparison, the average amount of improvement in a student's knowledge of economics after completion of a two-semester course is poor,[1] and (2) that even after a year of college economics the typical student does not really know or understand very many of the elementary principles to which he has been exposed.

With regard to what might be done to improve the level of college teaching in economics, Whitney specifically points to several areas where, in his judgment, we need greater teaching emphasis. We will not quarrel at length with this aspect of his analysis. Suffice to say judgment is involved in selecting the areas of economics that need to be emphasized and, more importantly, that the areas of "weakness" revealed by Whitney's testing may or may not be evidence of student or instructor inadequacies. It could be the tests that were at fault. With regard, however, to the more general conclusion that college students learn little from their first exposure to economics it is our view that: (1) Whitney's conclusion is probably much too pessimistic, and (2) had his analysis not been predicated on a wrong assumption his quantitative findings would have been much more encouraging.

To test the before-and-after knowledge of economics students Whitney used several sets of similarly constructed true-false questions. A score of two points was given for each correct answer and no penalty was assessed for wrong guesses. Significantly Whitney began with a score of 50 per cent equivalent to zero "Since guessing would, on average, yield a score of 50 . . ."[2] But this, in Whitney's words ". . . implies that a student has as many false as true notions about the questions being answered" [2, p. 160]. To us, the assumption that answers to questions not known were always randomly selected seemed dangerous.[3] Suppose instead, we hypothesized, that the "before" results flowed from almost no knowledge or understanding whatsoever of the economic issues being tested. Suppose in most cases these were chiefly the result of random selection. Suppose further that at the end of the course few answers were selected at random and most were selected on the basis of whatever economic reasoning the student was able to achieve, but that the questions, when answered in this fashion, typically failed to indicate the true progression of the student. If this hypothesis were valid, then an average score at the end of the course closely approximating that normally obtained at the beginning of the course should still mean progression from a relatively poor

[1] Whitney found an increase in understanding of about 20 per cent to be usual.

[2] Unfortunately Whitney often "forgets" that this is what he has done. For example, in the recapitulation, question by question, of some of the results of his findings he sometimes states, and repeatedly infers, that at the beginning so and so many students *knew* [italics ours] the answer and that during the course so and so many additional students learned the point.

[3] As is often the case, limited knowledge may lead one astray.

understanding of the principles tested to a relatively good understanding.

To test this hypothesis we must attempt to measure the progress in understanding that was not measured by the Whitney test. For this we would most prefer an essay-type examination. But this is not an approach that easily lends itself to the empirical method.[4] For this reason, as a compromise measure we settled on asking for a reason for the true (or false) answer selected by the student in each instance.[5] In doing this we used one of the test sets that Whitney had used and so were able to compare our results with his.[6]

Our raw scores (true or false, unadjusted by the student's reasons) were not markedly different from those Whitney obtained. From this we concluded that our group was about average. However, when we sought to evaluate the reasons students gave us for their answers we found, typically, that large numbers of additional questions had to be counted as wrong if the students had had no economic training, but that if the student had completed two semesters of economics he was much more likely to have an economically sound reason for his choice. Thus, when we sought to evaluate answers, we found a 50 per cent improvement in economic knowledge as contrasted with the 20 per cent improvement that Whitney found.

What does this tend to prove? Our results show that Whitney's quantitative findings are much too pessimistic. Our hypothesis that some of the Whitney questions induce misleading results seems valid. Our hypothesis provides a most reasonable explanation for some of Whitney's findings, and it seems to be further borne out by the kind of answers we often got to some of the questions asked.

As we examined Whitney's results we found a good many instances (42 out of 50 on one test set) where little or no improvement was achieved, and some cases (8 out of these 42) where retrogression occurred. Whitney attributes the latter to statistical error and the former to inadequate teaching methods. We reject both explanations. In the first place the fact that 5 questions of the 8, or 10 per cent of the test, show significant retrogression makes us inclined to doubt the validity of the test. But, we like even less the conclusion that a two-semester course in economics destroys a portion of the valid economic knowledge with which a student begins. In the second place, it also seems unreasonable to conclude that 80 per cent of what is attempted in the principles courses ends in near total failure. We much prefer to conclude that the examination was a bad one.

As we examined the answers we obtained on a Whitney test, we found some questions that were clearly misleading. In the case of one question, in fact,

[4] This is an objection that Whitney anticipates, and he also comments on the difficulty of evaluation of essay results. Whitney attempted to use the true-false-with-answer approach but, to the writers at least, his results are sufficiently unclear that we are unable to evaluate how closely they approximate our experiences.

[5] The possibility of a true-false examination with a strong penalty for wrong answers (5 to 1 for example) was considered, but discarded as likely to be less reliable in its results than the technique selected.

[6] Our test was applied only once, on a before-and-after basis. Obviously, therefore, our results are not conclusive. We do not have enough different student, professor, or test-set categories to get a truly random sample in any sense of the word. On the other hand, our procedure yielded results so markedly different from those Whitney obtained that we suspect that further testing would substantiate the general direction of our findings.

which really was a test of the Lubell thesis[7] [1], the wording apparently was unclear to the students. As a result, 25 out of 25 students who had had two semesters of economics appeared to miss the point. In other instances, as one might expect, we found cases where perfectly valid conclusions were reached on the basis of an inappropriate assumption. Again we found that a more detailed examination of test results indicated that the typical student understands a good deal more basic economics than Whitney's study would lead us to expect.

Quantitative evaluation of the progress of a college student on any basis except a comparative one seems doomed to futility. To test adequately the absolute progress of principles students, we must first find a test that measures absolute progress with reasonable accuracy. As any instructor knows, it probably would be possible to construct a test on which most students would score about as well at the beginning as at the end of the course; or to construct a test on which students would usually do poorly at the beginning and very well at the end, etc. To meet this problem we need a test that gives an accurate measure of progress in understanding. The simplest way to construct such a test would be to take students with known capabilities and develop a test on which "A" students typically perform well, "B" students not so well, and so on. But, if we adopt this procedure, we define our testing procedure circularly. Our results would be that, after two semesters of economics, good students do well, poor students do not, and the average student makes average progress. Whitney apparently does not fully appreciate this problem. He cites several possible objections to his testing procedure and then goes on to show that even granting the objections the direction of the results will not normally be affected. He misses the point. To the degree that criticisms of the test sets Whitney used have a bearing on his quantitative results the objections are highly significant. That is we accept the premise that students learn something in two semesters of economics; we want to know how much.

In contrast to Whitney we conclude that, lacking valid data to the contrary, the teaching of economics on the college level is satisfactory. We share the hope that it can be improved, but we consider economics to have an important enough place in college training so that, with or without measurable improvement in the teaching of this subject, the course should be retained. This is our value judgment. We would never pretend it is anything else.

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Richard B. Harshbarger

References

1. H. Lubell, "Effects of Redistribution of Income on Consumers Expenditures," *Am. Econ. Rev.*, March 1947, *37*, pp. 157-70.
2. S. N. Whitney, "Measuring the Success of the Elementary Course," *Am. Econ. Rev.*, March 1960, *50*, pp. 159-69.

[7] Question number 25 of Whitney's test set B reads as follows: "When the ratio of total wages to total interest plus profit expands, it is likely to result in an increase in the ratio of national consumption to national income." Clearly this question seeks an evaluation of the impact of income redistribution, not that of an income increase. Whitney rejects the Lubell thesis—i.e. he considers the statement to be true.

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Measuring the Success of the Elementary Course: Reply

I am grateful to the authors for their interest in the survey. Next summer I shall send a report to them and others participating or interested (including 35 teachers who have written regarding my earlier communication). May I request those giving the tests to separate the scores by questions according to sex?

The Rockwood-Harshbarger contribution has been to ask for reasons along with true or false answers. It appears that there were 50 per cent more correct answers supported by correct reasons after the two-semester course than before. This confirms the 46 per cent improvement in an earlier test [2, p. 167] in which 28 beginning students and 21 who had completed two semesters gave answers and reasons for the 25 even-numbered questions of set D.[1]

Should these results dispel any "pessimistic" outlook or "dismal prognosis"?[2] As I see it, the 50 (or 46) per cent gain is not an adequate measure of the progress of a class. If an original 30 per cent of good reasons for correct answers, for example, is raised to 45, this still says nothing about the remaining 55 per cent of wrong answers or wrong reasons at the end. An advance from 10 to 15, or even from 2 to 3, would also be 50 per cent. My own summaries, therefore, are in terms of per cent of potential gain (to 100). An advance from 30 to 45 would be 21.4 per cent of the potential, in comparison with 20.9 and 18.7 per cent for the median, and 21 and 17.4 per cent for the mean, of men's and women's colleges in my tests [2, p. 162.][3]

Tests asking for reasons have a real value, but it is not that of showing whether improved scores on objective questions correspond to improved understanding of the topics covered. They *must* correspond, if the sample is large enough. Zero knowledge, on our questions, would produce a score of 50;

[1] Rockwood and Harshbarger mention in footnote 4 that I did not describe this test clearly. I shall do so now. Good reasons for correct answers were marked 3 and weak reasons 1; irrelevant reasons for correct answers, and an equal number of wrong answers, were assumed to represent guesses (though undoubtedly many were offered with more or less confidence on the basis of what was thought to be reasoning); the remaining wrong answers were taken to represent not guesswork but fallacious reasoning. (We did not, however, analyze any of the reasons given for wrong answers.) Of the 700 beginning answers, 167 were accompanied by good, and 65 by weak, reasons; 113 were classified as right, and 113 as wrong, guesses; and the remainder, 242, as fallacious. Of the 525 answers at the end of the course, we counted 183 as having good, and 36 as having weak, reasons, 178 as being guesses, and 128 as fallacies. Good reasons increased from 24 to 35 per cent, weak reasons dropped from 9 to 7 per cent, guesses increased from 32 to 34 per cent, and fallacious reasons dropped from 35 to 24 per cent. The weighted validity of reasons given was 54.7 per cent for the 345 correct answers at the start, and 63.3 per cent for the 308 correct answers at the end. Another way of interpreting the results is that 11 per cent of answers shifted from fallacious to good reasoning, while 89 per cent were unchanged. Still another way is that nearly a third of the fallacious reasoning at the start was replaced by guessing, and about the same proportion of the original guesses by reasoning. If these interpretations are sound, the course at least dissipated a few false notions, in addition to its contribution to good reasoning. But the sample was small, and the detailed results depended on the teachers' subjective judgments of "good," "weak," and "irrelevant."

[2] I must plead not guilty to these charges. The elementary course *can* be improved—on some topics merely by "a few extra words in class" [2, p. 166].

[3] It was probably these figures from which Rockwood and Harshbarger deduced that the survey "found an increase in understanding of about 20 per cent" or a "20 per cent improvement."

complete knowledge would produce 100 were it not for imperfect phrasing of some questions and mistakes in answering due to carelessness and haste.[4] Improvement in reasons would be less than that in scores if students expressed good reasons badly or if they had learned some propositions by rote; it would be greater if teachers were too generous in judging reasons.[5]

Rockwood and Harshbarger say that some of my questions "induce misleading results" and that "perfectly valid conclusions were reached on the basis of an inappropriate assumption." I hope they will send me their evidence for aid in revision.[6] I can deal here with only the particular questions to which they refer. First, question B 25[7] is not in conflict with the Lubell thesis as I, at least, understood it in 1947 and reviewed it this week. His italicized conclusion is that "no redistribution of any feasible severity will bring about a large enough change in aggregate expenditures to offer a major contribution to the problem of increasing total demand" [1, p. 163]. Question B 25 does not ask how much the ratio of expenditures to income will increase, but whether it will increase at all. All of Lubell's examples [1, pp. 163, 165, 168] show an increase.[8]

Questions 25, 29 and 49 of set A [2, pp. 163-66] are three of the five on which there was enough retrogression during the course to make Rockwood and Harshbarger skeptical of the test itself.[9] But is it so unbelievable that cur-

[4] One teacher has taken set B, and another set D, in my presence; each took plenty of time and each scored 98. It appeared that we disagreed on one answer. I would expect a wider disagreement, and errors of carelessness and haste if there were a time limit, so that the usual teacher scores might be a few points lower.

[5] Thus I might "doubt the validity" of one test or the other as a result of such inconsistency in grades, but hardly as a result of low marks alone—even a "near total failure" on 42 out of 50 questions, in the words used by the commentators to characterize set A results.

[6] Some of the poorly worded questions had already been revised—for example, the original B 33: "One injustice in our economic system is that which is inflicted on college professors who receive lower salaries than business men of the same ability." If more carefully worded, this might have been a good question. At three men's colleges, however, scores at the end of the course averaged 36, 31 and 26; four groups of women scored 24, 19, 7 and 0. Although I admired the courage of the minority of students who thus told their teachers that their pay was adequate, I gladly acceded to the protest of the next college and moved to the safer ground of the influence of occupational risks on blue-collar wages. Scores improved: to 82, 77 and 56 for men, and 93, 80 and 73 for women.

[7] It is puzzling that the average score on B 25 dropped during the course at three out of five colleges. Including some colleges which gave it only before, or only after, the course, the average men's score dropped from 77 to 68, and the women's score from 81 to 73. Evidently most students had "known, sensed or guessed the answer at the start" [2, p. 165]—this being the correct phrase, rather than "knew," which is rightly criticized in footnote 2 of the comment. The particular wrong reasons given by the 25 members of the Rockwood-Harshbarger student group may shed light on why some lost this assurance.

[8] In terms of third-quarter 1960 consumer spending, $325 to $330 billion, the three models used by Lubell for his mildest suggested redistribution would result in increased spending of $1.6, $1.6 and $9.5 billion, respectively. As he says, the third increase is "certainly significant" [1, p. 168].

[9] Since March I have received another report [see also 2, p. 166] of the use of before-and-after tests in an advanced course. A class in "public control of business," achieved 38 per cent of its potential gain. This is a better result than in any of the elementary tests—35.7 per cent, on set E, at one of the older women's colleges in the Northeast, being now the best.

rent teaching approaches might have unsettled, for some students, the common-sense judgments originally held by a majority that dividends can be spent as well as saved, that depressions are not the times when new highways and post offices will be most used, and that the excise tax is a burden on sellers as well as buyers of gasoline?

Rockwood and Harshbarger would like to see a test on which A students typically perform well, B students less well, and so on; but reject as circular procedure the use of those questions on which A students have been found to perform best. It would at least be a short cut, and sometimes more reliable than essay questions, in recognizing future A students. But my tests were not developed in that way. I merely chose questions a person trained in economics ought to be able to answer. Later one college sent me final examination scores, and a correlation did appear [2, p. 168.][10]

The "value judgment" that the full elementary course, whatever its weakness, should be continued is one to which I subscribe wholeheartedly. That is why the aim of this survey is its improvement. Too many teachers have written me that "our course has been reduced to one semester, and I fear our students would make a poor showing on your tests" or "we cover only selected areas, whereas your tests attempt to cover the whole subject."[11]

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However, answers to all the "public control" questions were in the textbook or lectures, whereas many questions in the elementary tests are not so covered. In general, different considerations control the progress in institutional and in more theoretical courses.

[10] A high correlation between true-and-false scores and grades in the course would mean that the scores confirmed the effectiveness of the course shown by the grades. At this college the correlation ratio between individual test scores and examination grades was only .39. The rank correlation of the 21 test scores (80-40) and corresponding averages of grades was .99. The mean of test scores was 66.3, that of examination grades 67.3. The students achieved a final average mark of C for the course, but only because these examination grades (realistic though they were, according to the objective tests) received a low weight. This C implied that the students had learned about three-quarters of the material; whereas the test score of 66.3, compared with 56.3 for this group at the beginning, implied that only 23 per cent (10.0 ÷ 43.7) of the information represented by the 50 questions, and not already known, was learned. Presumed student knowledge had more than doubled (from 6.3 to 16.3).

[11] I cannot, however, claim either that my test questions cover all important subjects in elementary economics or that they are all on the same level of importance. Suggestions for better questions will be welcomed.

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BOOK REVIEWS

General Economics; Methodology

The Political Economy of National Security. By JAMES R. SCHLESINGER. New York: Praeger, 1960. Pp. vii, 292. $5.00.

This is a pioneering book. It is an attempt to come to grips with the rapidly changing economic aspects of national security problems. The attempt is overdue: economists ought to be speaking out on this range of problems much more than they are. For this reason, among others, Professor Schlesinger's book is most welcome. It is the first systematic treatment of this area, the beginning, it is hoped, of a body of literature that can supply much-needed guidance to policy makers.

Having said this, I must go on to say that in my view the book is wide of the mark in many respects. It is, I suppose, inevitable that there should be disagreement over what a book on a new subject should cover; in this case I feel Schlesinger has written competently about a number of problems, but that many are not really relevant; and he has left out a few of the most significant.

He begins with a most satisfactory treatment of why the economist should be interested in national security, and a first-rate simplified version of the uses and limitations of gross national product as a measure of economic power. He then describes the concept of economic potential for war (EPW) as the difference between potential GNP and subsistence for the population. He advances this concept by bringing in the relevance of the kinds of industry a country has: dollar for dollar of output, machinery yields more power than services, at least in the intermediate run. All this is satisfactory, and relevant to a war like the second world war. But what of nuclear war? Here he asserts (p. 64) that "the role of economic capacity in nuclear war is rather nebulous. It is doubtful whether it would influence the fighting, which would be settled by the forces-in-being." In this same connection the adjective "nebulous" appears twice more (pp. 75, 177).

Far from being nebulous, it seems to me that the connection between economic power and nuclear war is central, direct, critical, of overwhelming importance, and indeed the main subject to which a book with this title should be addressed. For economic power or capacity is one of the chief determinants of the strategic position of military powers; it supplies the wherewithal for protecting the deterrent, protecting the population, staying ahead of or even in the scientific race, helping allies, keeping forces modern, achieving the strength from which realistic arms control negotiations can go forward, and harnessing space; in short, wise use of our resources determines whether we survive in a hostile world. And on most of these issues I find nothing in the book (civil defense, for example, is not mentioned), except for the following sentence (p. 178): "After major powers have reached the plateau of industrial plenty, differences in relative economic capacity provide no strategic advantage, aside from the issues of the disposition of that capacity, and its relative vulnerability to attack."

To what then does Schlesinger devote his attention? There is a lengthy

chapter on problems of economic mobilization which surely has for the most part only historical interest. General or selective controls, the impact of controls on small vs. large business—are these really relevant issues today? Is it really a "vital question" (p. 80) whether price or wage controls should be instituted first?

There are chapters on budgetary planning, on international trade, on what makes underdeveloped countries grow. These are competent essays; the author makes particularly good sense when talking about foreign aid, its aims, its successes and failures. But the mortar is somehow missing; the relevance of these important problems to national security is not made apparent. Chapters on general aspects of international trade, or economic growth, seem not to be closely tied to the over-all subject. Furthermore many of these subjects of necessity are treated briefly; hence they suffer from failure to qualify, and from a tendency to assert rather than demonstrate (see for example the assertion of the need for growth "in a balanced way" on page 195, surely an arguable proposition).

In the chapter on Soviet economic growth, Schlesinger tries hard to strike a balance between the high-growth-rate and the low-growth-rate "schools." But he does this mostly by averaging, an unsatisfactory technique at best. In many ways this chapter is his poorest, which can be excused, I suppose, since the subject matter is both strange and difficult. On what the differential growth rates (whatever they are) really mean to U.S. policy, however, he has some very sensible things to say.

It is encouraging to find people of Schlesinger's ability writing on national security issues. He has given us a lot to chew on; and if he has emphasized some aspects of the problem which in my view seem less vital and omitted others which seem more so, this is surely not crucial. The important thing is that he has taken the plunge; it is to be hoped that others will join him.

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Der Wettstreit zwischen Mikro- und Makrotheorien in der Nationalökonomie. By FRITZ MACHLUP. Tübingen: J. C. B. Mohr (Paul Siebeck), 1960. Pp. 55. DM 5.—.

For some time Fritz Machlup has carried on a crusade against economic "weaselwords" demonstrating, for example, the imprecise use to which concepts, such as "structure," "statics" and "dynamics," or "equilibrium" and "disequilibrium" have been put in contemporary literature. In these lectures delivered to a German audience, he now exposes some of the ambiguities which attach to the notions of "microeconomics" and "macroeconomics," trying at the same time to evaluate the relative significance of these two approaches for theory formation generally.

It has not been the author's intention to break new ground, or even to discuss analytical intricacies, *e.g.*, those concerned with the aggregation problem. Rather he defends a common-sense solution, in which the view of the economic process as derived from the behavior of the "parts" naturally supplements the results yielded by observation and analysis of the "wholes." The dif-

ficulties begin, and Machlup is fully aware of them, when we try to determine what is part and what is whole. None of the definitions which he distills from the literature—distinguishing the micro- and the macrosphere according to subjects of decision-making or the manner of aggregation or the role which price relations play—prove very helpful. Are the planning decisions in a collectivist system macrophenomena because they refer to a comprehensive economic whole, or are they microphenomena since, as decisions, they emanate at least ideally from one will? Conversely, how are we to place a Walrasian general equilibrium?

We are unlikely to find satisfactory answers to such questions unless we are willing to reconsider the manner in which the postulate of "methodological individualism" has been formulated by the Austrian School, on which Machlup rests his case for the primacy of microeconomics. Such reconsideration amounts, above all, to distinguishing between the "molecular" source of all economic decision-making, and the quite diverse magnitudes of the economic units to which such decisions can refer. Only in the model of a *laisser-faire* system may we treat also these units as molecular, namely as representing the ultimate "particles": households and firms. There microeconomics can indeed be defined as dealing with the constituent elements of the economic total as well as with the actual "forces." And macroeconomics is then confined to the study of "structures," namely of the mutual relations among the constituent parts of the system as they derive from the interplay of individual decisions. On what level of aggregation these relations are studied poses difficult statistical but hardly any theoretical problems. However, as soon as we admit into our models policy-making behavior, some of the effective forces are directly concerned with the "molar" order of the economic process, and the "structures" are no longer an unwilled resultant but the purposive creation of decision-making.

Machlup's brief survey of the history of doctrines—from which Marx, the inventor of the investment-consumption model, is unfortunately absent—leads to the conclusion that, in the past, the distinction between microeconomics and macroeconomics practically coincided with the difference between "force analysis" and "structure analysis" as characteristic of a pure market system. This changed for a while when aggregate consumption functions or supply functions were treated as if they described the behavior of original forces. With the pendulum swinging back to the consideration of the true sources of decision-making, the need for unambiguous concepts applicable to mixed systems of economic control is greater than ever.

Even if it does not provide the answer, Machlup's exposition points to the real questions. Besides, in disposing of many undue claims which have been made on behalf of conventional macroeconomics, he has useful comments to offer on a number of incidental issues, such as the meaning of equilibrium, the distinction between *ex ante* and *ex post* magnitudes, or the conceptual difficulties in establishing behavioral relations. There is also an interesting section in which the concept of the firm, which is appropriate for micro-theory, is contrasted with that conventionally applied in business economics.

Adolph Lowe

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Kratkii ekonomicheskii slovar. (Short Economic Dictionary.) Edited by G. A. Kozlov and S. P. Pervushin. Moscow: Gospolitizdat, 1958. Pp. 391. 11 Rbl.

Among the problems facing the student of Soviet economics few have been as intractable as the search for the real meaning of concepts and slogans used by Soviet economists and propagandists. The difficulties encountered in this field have not been confined to the non-Communist world. Soviet scholars too have been periodically criticized in Communist journals for their inability to interpret satisfactorily Marxian economics and their application in the USSR.

The purpose of the *Short Economic Dictionary* is to guide Soviet economists. Printed in 450,000 copies, it is the collective effort of 70 Soviet economists. It consists of over 800 entries which vary in length and amount of detail. The entries are placed in alphabetical order and are buttressed in some cases with short quotations from the writings of Marx, Engels and Lenin.

The bulk of the entries consists of definitions and explanations of concepts and slogans embracing every aspect of political economy in the broad sense of the term. Though more space is devoted to concepts used in the Soviet rather than the capitalist economy, the latter is not neglected. On the other hand, the criticism meted out to different aspects of capitalism is not extended to Soviet economic institutions and policies, past or present.

The *Dictionary* also contains a certain number of biographical entries. These are weighted heavily in favor of socialist thinkers and economists with special emphasis on those who were active in czarist Russia. Western economists are dealt with in a variety of ways. Some of those who received favorable mention in the Marxian classics get more sympathetic treatment than, for instance, Keynes who is described as "the ideologist of monopoly capitalism and imperialist reaction" whose theories "the agents of the bourgeoisie in the labor movement are trying to use" to "deceive the working class." G. D. H. Cole is referred to as a "Right Labourite" and J. A. Hobson as "a typical representative of bourgeois reformism." Only four American economists (H. C. Carey, J. B. Clark, A. H. Hansen and Seymour Harris) are considered worthy of inclusion in the *Dictionary*, though their views hardly meet with the approval of their Soviet biographers.

Ivan Avakumovic

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Economics and the Modern World. By Lawrence Abbott. New York: Harcourt Brace and Co., 1960. Pp. xiv, 880. $6.95.

This is an excellent text. However, the justification for any new one in an already crowded field depends on its comparative advantage; *i.e.*, the breadth of the need it purports to fulfill and its likely success in meeting that need. Abbott's book is thus reviewed on the basis of five key features suggested in the preface: (1) unique organization which allows introductory price theory as well as national income analysis to come early in the course; (2) careful definition of terms when first introduced, with subsequent listing in the index for a handy dictionary of terms; (3) integration of fact, theory and policy with emphasis on economic theory as a practical tool; (4) clear separation of

value judgments from facts and theories; (5) theoretical parts pitched to the level of freshmen and sophomores.

The text's organization is not unconventional, yet has a few unique features. The five chapters of Part I provide an "introductory view" similar to other texts. Squibs from newspaper and magazine articles are interwoven with analysis to describe the broad scope of modern economic problems. Part II includes four chapters on "markets and prices." This is a desirable arrangement in that the student is introduced early to this basic economic theme. Yet his interest is not choked off by an overdose of the theory of the firm before he studies macroeconomics with its interesting institutional and policy aspects (Part III, Ch. 10-19). These 19 chapters provide ideal coverage for one semester, but subdivision would be difficult for courses operated on the quarter system.

Parts IV and V (7 chapters each) cover conventional neoclassical economics, but not in stereotyped fashion. The interspersed chapters on antitrust, public utilities, organized labor, taxation, and income inequality enliven what for many beginning students is generally unpalatable material.

Part VI contains 4 chapters on "the world and the future." As with most other texts, these most vital issues, about which students are inherently most excited, are saved to the end. Possibly economists are caught in a dilemma, but one would like to see some textbook writer innovate by discussing trade barriers, economic growth, underdeveloped countries, the population problem, and economic systems in the beginning of his book. Do students really need more "background in theory" before these issues can be properly understood? It is doubted if their interest can be suspended from September to May!

The purported clear definition of terms, along with use of the index as a handy "dictionary of terms," does not test very high. In many cases considerable searching in the text was required before either the term or definition could be found. Then, all too often, the term was not defined in crisp fashion. The dictionary device could be much improved if both the term and definition were italicized or underlined on their initial appearance.

Abbott does an excellent job of integrating fact, theory, and policy. The text is replete with cogent examples at each step along the analytical path, reenforcing the author's proposition that economic theory is a practical tool. He is painstaking about stipulating assumptions in building his models and indicating modifications for particular applications.

In some, but not all, policy chapters the author specifies his own opinions of the proper goals to be pursued and the value judgments on which they rest. He dares the student to differ with him and then evaluate whose judgment compares most favorably with that of U.S. society as a whole. These sections should provide ample grist for fruitful class discussion, particularly chapters on agriculture, antitrust, and unequal income distribution. Through these snapshots an image of Abbott's economic philosophy emerges as a classical 19th century liberal (freedom of individual choice) combined with a 20th century liberal (considerable government intervention in creating the context within which economic activity unfolds).

Although the book purports to be pitched to the level of college freshman

and sophomores, it will prove to be an overwhelming task for freshmen and a spirited challenge for sophomores. The theory portions are difficult, not so much because they are so advanced (no isoquants, for example), but rather because they tend to be so encyclopedic. One gets the impression that Abbott doesn't want to leave one stone unturned, particularly in the chapters on national income determination (Ch. 13), theory of money (Ch. 14) and pure competition (Ch. 21).

The charts and tables are generally instructive, some representing pedagogical innovations, such as the "marginal utility wheel" (p. 135). A few, unfortunately, suffer from faulty construction or comparisons (especially pp. 258, 343, 416, and 741). These may be misleading unless clarified by the teacher.

The minor detractions mentioned above notwithstanding, *Economics and the Modern World* should find wide usage. Professors desiring to teach a rigorous principles course should not overlook this book. Those teaching survey or general education courses had better look elsewhere.

FRANK W. GERY

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Elementary Economics: Principles, Problems and Policies. By CAMPBELL R. MCCONNELL. New York: McGraw-Hill Book Co., 1960. Pp. viii, 759. $6.95.

Elementary Economics is an exceptionally well-written and well-organized text for introductory courses in economics. McConnell presents abundant historical, institutional, and policy-oriented material as well as rigorous theory, which is made stimulating and believable through lucid and carefully developed explanations and convincing examples. Also, the book is one of the few introductory texts to do justice to the ideas of J. K. Galbraith and to the subject of economic development.

The first section, "An Introduction to American Capitalism," stresses the nature and methodology of economics and the problem of economizing. The principle of the allocation of scarce resources among alternative human wants is given full expression in chapters on the nature of capitalism, the economic functions of government, and households as income receivers and spenders. Indeed, throughout the book the author succeeds in making the economizing problem the unifying principle. Thus for the student economics comes to be a way of thinking and not merely a collection of economic topics. Supply and demand analysis is introduced early in the book and serves to clarify the general operation of the price system which is explained in this first section. The circular flow of wealth, though expanded in a later chapter, is presented in Chapter 3 which suffers slightly from careless editing.

In the second section, covering national income, employment, and fiscal policy, McConnell succeeds in avoiding any artificial distinction between national income analysis and business cycle analysis. The chapter on national income accounting is as clear and interesting as most text presentations except that the student is not likely to understand without further explanation why or how household savings flow into the business investment stream. Classical

employment theory is well constructed and then well torn down. The explanation of "the new economics" is clear and convincing and does not suffer at all from the simplification required of an introductory treatment. McConnell's linear consumption function will annoy economists who still hold that the marginal propensity to consume declines as income increases. Fiscal policy is given detailed coverage and evaluation.

The third section contains what the reviewer considers to be the only flaw of organization in the book: a chapter on U.S. economic growth follows three chapters on money and banking when it might be better placed in the fifth section of the book dealing with current domestic problems. A separate chapter on bank creation of money, replete with the type of balance sheets one would expect to find, follows a chapter on money and the banking system and provides a sound introduction to monetary policy which is given thorough treatment.

Part IV presents price and distribution theory. The first chapter of this section, describing in general terms the basic market models of pure competition, pure monopoly, monopolistic competition, and oligopoly, is of limited value since later in this section a separate chapter is devoted to each of these market models. Supply and demand analysis, explained earlier in the book, is reviewed briefly, and elasticity is given full elaboration. An optional chapter explains the downsloping nature of the demand curve and the theory of consumer behavior via marginal utility analysis but with no indifference analysis. Supply and the costs of production are accorded detailed explanation. The chapters on market structure are sophisticated, clear analyses with life infused into the abstract models by a good selection of market data. Also, each type of market structure is evaluated in terms of efficiency, rate of technological progress, and social implications. Distribution theory is confined to three chapters: the first explains the derived demand for resources, the second explains wage determination, and the third lumps rent, interest, and profits together giving a brief orientation to these factor returns. This condensation will undoubtedly disturb some theorists but will be welcomed by others, like the reviewer, who are embarrassed year after year explaining at length theories of distribution which are inadequate. This section concludes with a perceptive evaluation of U.S. capitalism.

The fifth section examines, with the aid of tools of analysis already developed, the current domestic problems of monopoly, agriculture, unions, and economic inequality and insecurity.

The last section of the book is concerned with international economics. When reading the two chapters on international trade the student may find it awkward having to go back to the beginning of the book (Ch. 3) to read about the gains from trade and the basis for comparative advantage. A good treatment is given of the problems of underdeveloped countries and a whole chapter is devoted to the economic challenge of Soviet Russia, but at the expense of some attention to other economic systems.

Elementary Economics has minor flaws but no major one; and this reviewer believes that the careful development of the book, the clarity and forcefulness of the explanations, and the stimulation which students will derive from the book more than compensate for the minor flaws. The questions

at the end of each chapter are excellent. A student workbook supplements the text and teachers may benefit from the instructor's manual.

STEWART E. BUTLER

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Essays in Economics and Econometrics—A Volume in Honor of Harold Hotelling. Edited by RALPH W. PFOUTS. Chapel Hill: University of North Carolina Press, 1960. Pp. ix, 240. $7.50.

The economists' respect for the contributions of Harold Hotelling is indicated by this volume of essays written by Arrow, Davis, Ferguson, Friedman, Frisch, Hurwicz, Klein, Pfouts, Samuelson, Tintner, and Vickrey. Perhaps no man has been honored with so impressive a list of contributors, all of whom were former students or associates. And like the work of Hotelling the contributed papers deal with difficult problems in economic theory by means of relatively advanced mathematics.

Samuelson's opening essay on the structure of an equilibrium system is an exhaustive summary of its properties in exceptionally concise and abstract form. This paper follows the lines of analysis presented in his *Foundations*. Since equilibrium systems characterize the method of analysis of all the sciences, it is not surprising that this essay is a discussion of the logic of analytic methods. Unfortunately for this reviewer the level of analysis is so advanced mathematically that he has had to accept the pedigree of the author as evidence of its validity.

The most impressive essay is co-authored by Arrow and Hurwicz. Following Samuelson's conversion of equilibrium to maximization problems, Arrow and Hurwicz continue by presenting a solution to the minimax problem of game theory—a problem that arises because a constrained maximization can be converted to a minimax. Earlier contributions of Arrow and Hurwicz to the gradient method of finding a minimax are related to the convergence to equilibrium in a decentralized system. They provide an economic, institutional interpretation to the gradient method, not only in the case of increasing costs but for decreasing costs as well. Perhaps more interesting to economists, judging by the reviewer's personal experience, is their presentation of methods of converting constrained maximum problems to saddlepoint or minimax problems by means of "concavified Lagrangian functions." This essay like many others is at a very high level of abstraction. As the authors warn, any implications about optimality or criteria of performance in the real world are limited to the case of a single-person utility function, rather than to a society of many individuals.

Vickrey and Ferguson attack practically the identical question with identical results. The issue is Edgeworth's so-called taxation paradox wherein an excise tax on one commodity can result in lower prices. Building on Hotelling's contribution, both authors present nearly identical analyses. Whether their papers are complements or substitutes is here as moot as for most economic goods. Ferguson generalizes to a wider range of possible results, where-

as Vickrey suggests two economic examples where the "paradox" might occur. At the risk of being completely wrong, this reviewer's interpretation of their analyses is that a necessary condition for at least one of the prices to fall is that either (1) the rise in price of commodity x produces a greater excess supply of commodity y than of x, or (2) the cross price-elasticity exceeds the own price-elasticity in demand and in supply! Both are hard to swallow. Is my intellectual throat small, or the morsel oversized, or my sense of taste inadequately developed? In any event this pair of essays found the warmest welcome.

Tintner and Pfouts contribute two closely related essays. Pfouts introduces hours of work and savings as variables in a person's utility; Tintner introduces other people's consumption and income into each person's utility function. As in the case of the simpler utility functions all that can be said about the effects of a price change on quantities consumed is "an income and a substitution effect" with nothing definite about the resultant direction of effect. This holds for the interpersonal utility function of Tintner as well as for the hours-of-work and savings variables in Pfouts' analysis. It is still true that empirically refutable implications require restrictive postulates—something that some economists seem hesitant to acknowledge in the mistaken belief that one shouldn't rule out all possible kinds of behavior.

Harold Davis' paper is an extension of his earlier logical excursion into the analysis of time series of prices by means of differential equations. Frisch presents a series of definitions of input-output coefficients. These are expressed in physical, in volume, semivolume, and current value units. Space prohibits presenting the details here. Until Frisch presents a more detailed explanation of the implications of the various alternatives for input-output analysis, the results of the present essay will remain purely classificatory.

Lawrence Klein compares the efficiency and bias of various methods of estimating coefficients of an equation. He first presents an illuminating discussion of the difference between the estimates of the multiplier, for example, derived from an estimate of the simple consumption equation (the structural equation) and the estimate of the multiplier derived from (the reduced-form equation of) a system of equations. The first can be thought of as a partial analysis model, and the latter as a total model analysis. Thereafter he presents logical and empirical analyses to compare efficiencies of reduced-form equation coefficient estimates with and without a prior imposed restriction. The empirical sampling results cited support the logical analysis.

Possibly beneficial effects of destabilizing speculation are developed by Friedman in the only paper devoid of any mathematics and almost the only one not requiring extensive familiarity with matrix theory. Friedman explores some implications of the conjecture that facing or bearing uncertainty is a sort of activity for which some people are willing to pay a price (because they like to be exposed to uncertainty). He shows that if destabilizing speculation occured frequently, it could be the result of such preferences.

Professor Hotelling's impressive bibliography (to 1958) concludes the volume.

Armen A. Alchian

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Productivity and Technical Change. By W. E. G. Salter. Monograph No. 6, Department of Applied Economics, University of Cambridge. New York: Cambridge University Press, 1960. Pp. xii, 198. $4.50.

No one doubts that technical progress is primarily responsible whenever large increases in industrial productivity are observed. But what are the processes whereby scientific and technical advances are converted from potential to actual gains in output per man? What economic factors determine the rate at which new techniques are adopted? How do factor prices affect the development and adoption of new techniques? Why does productivity per man vary so widely from firm to firm? Why are increases in productivity so unevenly distributed among industries?

Salter makes a brilliant attack on these and related questions, utilizing both a theoretical analysis and an empirical study of 28 British industries during the 26 years ending in 1950. The theoretical analysis developed originally as an aid to the interpretation of his empirical results, but it has become a major contribution and stands independently as the first half of the book. Here is a theory of production in which changing technical knowledge and factor prices determine the best-practice techniques available at any point of time, while the rate of gross investment, itself a function of the relative prices of labor and of real investment, is a major determinant of the speed with which new techniques are adopted. (A larger portion of existing equipment is regarded as obsolete when real wages and the flow of replacement investments are high than when they are low.)

Salter avoids classifying innovations under the misleading labels of "labor-saving" or "capital-saving." Instead he reminds us that most innovations save both labor and capital, and defines a measure of the labor- or capital-saving *bias* of technical advance which he finds more appropriate for our use.

Salter has a happy faculty for building a penetrating analysis from simple theoretical materials. He writes with clarity and balance, presenting his arguments with due caution, but not submerging them hopelessly in a mire of qualifications.[1]

Part II of the book reports on an interindustry survey of output, employment, productivity and unit costs in 28 British industries between 1924 and 1950. There have been marked differences among the increases in labor productivity in the various industries. These differences have not been associated with differences in the earnings of labor but with differences in unit costs. The industries showing the greatest increases in output per head also tend to show the smallest increases in selling prices and the greatest increases in total output and employment. The last chapter of Part II contains a similar statistical survey of productivity experience in the United States. The results of the British study are confirmed.

Along with these differential changes in labor productivity there has been a marked change in the structure of output, employment and relative prices since 1924. Salter believes that the primary causes of this structural change have been an uneven rate of technical progress in the various industries and

[1] At one point (p. 39) some confusion is caused by the substitution of "growth" for "decline" when introducing the formulae for the rates of decline of unit labor and capital requirements.

the uneven impact of economies of scale. Factor substitution may have been a contributing factor, but could not have been the principal cause. (Some factor substitution is expected to result from progress, occurring in response to the cheapening of capital goods relative to labor which is brought about by increased productivity in the capital-goods industries.)

An examination of the interindustry structural changes that have been associated with these productivity movements leads Salter to recommend that public policy should hinder neither the growth nor the decline of individual industries: "a flexible structure of production is an important element in a high rate of productivity increase" (p. 9).

Salter has given us an illuminating and realistic analysis of the processes of growth, making effective use of both empirical and theoretical materials. This volume should be a significant work in its area for many years.

Victor E. Smith

Michigan State University

Interindustry Economics. By Hollis B. Chenery and Paul G. Clark. New York: John Wiley, 1959. Pp. xv, 345. $7.95.

The authors of this book have set themselves the goal of combining theoretical and practical aspects of interindustry economics in one short volume. They have not provided many links between application and the more theoretical portions of the text. Where they have made such an attempt the "application" takes on a fairly unrealistic flavor involving substitution of labor for capital, isoquants for the whole economy, etc. This is to be expected, since there is an inevitable lag between theory and application and only the simplest of interindustry models have been applied.

Each portion of the text serves a function of its own, however. Part I provides an exposition of input-output analysis, indicating the relationships between input-output models, linear programming, and economic theory in considerable detail. A few numerical examples, accompanied by graphical illustrations, provide the principal means of demonstrating the basic concepts. The linear programming analog of the transformation function or production possibility curve is presented as well as the production isoquants giving alternative ways of producing the same outputs. The "substitution theorem," that the particular combination of final demands has no effect on the choice of activities in the single-factor case, is also demonstrated by a graphical example.

The algebraic exposition is not always as clear as the graphical, particularly in the exposition of the revised simplex method which has been presented more clearly by others. Also, this reader is puzzled as to why "proportionality is not important" in the input-output model (p. 88).

The book acquaints the reader with the considerable difficulities involved both in constructing and in evaluating input-output models. The various attempts that have been made to evaluate particular input-output projections are described. The authors' own views appear somewhat more optimistic than those suggested by the empirical tests they cite. Their reasons are that better data will be available for future models, and, more important, that the

tests have not been concerned with the type of application where input-output techniques are most useful, such as problems involving a drastic change in the structure of an economy (mobilization or economic development, for example). The best test-case to date is perhaps the "U.S. Emergency Model" dealing with the rearmament program stemming from the Korean War. However, these results are still classified security information. In addition the authors contend that, typically, equivalent alternative studies using conventional techniques cannot be compared with interindustry studies because conventional techniques simply do not exist that can answer the same questions. As one example they cite a model for offshore procurement in Italy which required a fairly detailed commodity composition of import requirements. By what techniques other than interindustry analysis could this be estimated?

The authors are well acquainted with the literature and provide the reader with an overview of what has been attemped in applying interindustry analysis that would take considerable time to acquire independently. The book could serve as a text in policy-oriented courses in economics, providing a survey of theory and practical applications that is readable by those not specializing in interindustry economics or econometrics. It includes a discussion of a wider range of potential applications than is available in other input-output literature.

T. M. Whitin

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Economic Theory and Organizational Analysis. By Harvey Leibenstein. New York: Harper and Brothers, 1960. Pp. x, 349. $6.00.

This book uses a novel approach to the theory of the firm to attempt an extension of microeconomic theory. A theory of organization is developed to explain motivation and operations within the firm and hence actions of the firm in the industry. This is certainly in contrast to the traditional theory of the firm in which the internal functioning of the firm is described in only a primitive way.

To begin his fluoroscopic study of microeconomics, the author reviews several important areas in this field. The traditional theory of utility and demand, the theory of competitive equilibrium in both the short run and the long run, the theory of monopoly and monopolistic competition and oligopoly are all covered in a space of 85 pages. In spite of the brevity of the treatment, the exposition is lucid and essentials are covered with care.

The second of the four sections is devoted to defining and applying a concept of specialization. Leibenstein's concept of specialization is stated in terms of activities within production processes. If a group of individuals perform a set of activities in one process which are contained in a set of activities performed by a group of individuals in a second process, then we have a clear indication of greater specialization in the first process because fewer activities are performed by the group in the first process. But the indication is not conclusive because there may be other groups in the first process that are less specialized than some groups in the second process. Hence the problem of com-

paring the degree of specialization between two processes becomes logically similar to the index number problem. The material on specialization is interesting per se and, so far as the reviewer could see, is logically sound, but it seems to be only implicitly related to the remainder of the book.

The largest and most discursive section deals with a theory of organizations. The fundamental construct of the theory is the role or the role-player. The author is careful to distinguish his concept of the role from the usual sociopsychological concept of a role. To the author a role is made up of fields of action, *i.e.*, alternative actions open to the player of a role. Thus the primitives of the theory are fields of action and it is not reasonable to attempt to define or delimit these too closely. The role must be interpreted by the player, and interpretation of the role may (in some instance certainly would) differ from one player to another. Interpretation of the role means deciding what fields of action are appropriate to the role.

Obviously there are many types of role at many different levels. Routine performance roles call for little, if any, interpretation, while roles concerned with policy matters are open to a range of interpretation. The author provides a classification and analysis of role-types.

An organization is defined as a role structure that includes a working group of role players and players that determine such things as the roles to be included in the structure, the players to fill the roles and the payoffs to be made to all players. A firm is defined as an organization whose existence depends on the size of the aggregate payoff, the payoff being derived from the sales of goods and services. This definition offers scope for the author to develop a concept of organizational equilibrium and to rephrase many topics, such as efficiency and scale, motivation, etc., in terms of his organizational theory.

The final section deals or purports to deal with the implications of organizational analysis for microeconomic theory. In this section many interesting and significant observations, especially on the shortcomings of existing economic theory, are made and some suggestions for improving the theory are offered. In spite of evidences of original thinking, this section, which should be the glory of the book, is disappointing. There appear to be two reasons for this. First, the highly specific organizational theory developed in earlier portions is not used to any extent in the last section; this section could stand without reference to the first part of the book. Secondly the models and apparatus of thought are not fully worked out. Many hopeful suggestions are made and the reasoning is carried to a certain stage, but never as far as we might rightfully expect from the author. This is especially clear in the final chapter on the oligopoly problem.

But it is the independence of the last section from earlier sections that is most disturbing. Indeed the bride, microeconomics, refuses to unite with the groom, organizational theory, in a satisfying way in spite of the angry wagging of the shotgun held by Leibenstein, who appears to be the father of the groom.

In a more pertinent vein it may be observed that this book is a contribution to a recent literature of protest and suggestion about the state of microeconomics. Among the longer and later contributions those of Shubik and Baumol come to mind immediately. At the very least it seems that Leibenstein has

suggested an approach that is worthy of serious consideration and that may prove fruitful in the future.

RALPH W. PFOUTS

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The Powerful Consumer: Psychological Studies of the American Economy. By GEORGE KATONA. New York: McGraw-Hill, 1960. Pp. ix, 276. $6.50.

This book has much in common with the author's earlier book, *Psychological Analyses of Economic Behavior*, 1951. Both books are primarily concerned with consumer behavior and attitudes with respect to durable goods and savings, as revealed by surveys made by the Survey Research Center of the University of Michigan. This book has benefited from the vastly increased stock of information accumulated from these surveys over many years and from additional experience of the author and other research workers in developing methods of data correlation, in ferreting out conditions associated with the behavior being investigated, and from testing the validity of various hypotheses, for example, the effect of change in income or price, holding constant the effect of change in expectation as to future income and price.

The book has four main sections, namely, Introduction, Attitude Change, Psychological Findings, and Economic Fluctuation. Each of these includes some general discussion of psychological theories, some findings from surveys and some attempts at interpretation of the implications of the evidence. The facts are marshalled in such dense detail that many readers may lose sight of the relevant conclusions. In the interpretation of evidence some use is made of national time series. This constitutes, however, a minor part of the book, and it suffers somewhat from conceptual confusion; for example, in the discussion of savings and the permanent-income hypothesis.

The book is primarily addressed to economists who, by and large, are judged to be ignorant of basic psychological principles and prone to rely on outmoded concepts of economic man. The introductory sentence sounds the keynote of much that follows: "Instability of the economy is still one of our most pressing problems" (p. 3). Later the author claims that : "Information on changes in consumer sentiment contributes . . . to our ability to predict the future course of the economy" (pp. 8-9). Yet in summarizing evidence on the "index of consumers attitudes and samples of consumer durable goods" of the years 1952-1959, the author concludes: "Sometimes attitudes change autonomously and indicate changes in demand for consumer durables which cannot be explained by changes in income and other traditional financial data; sometimes attitudes are influenced by past developments in incomes, production, sales and the like, and do not contribute significant new information . . ." (p. 52).

Nevertheless the general review of findings of surveys made at the Survey Research Center presented in this book should prove useful to sellers of durable goods and of insurance policies wanting insight as to conditions likely to transform potential into actual buyers. In other words, it provides some guide as to probable response to selling efforts.

"Powerful" in the title appears to connote the author's judgment that some

consumers' discretion exists and that where there is discretion there is power. He emphasizes that willingness as well as ability to buy is important. The frequency with which the author has to qualify his findings as tentative and to point to "other" conditions that may be present makes it obvious that he feels that conditions affecting willingness are not yet well understood. He leaves little doubt, however, that in the expansion of this field of knowledge there is at least one enthusiast.

MARGARET G. REID

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The Demand for Durable Goods. Edited by ARNOLD C. HARBERGER. Chicago: University of Chicago Press, 1960. Pp. vi, 274. $5.00.

This compact volume contains five demand studies on major durable goods, including housing, refrigerators, automobiles, farm tractors and plant and equipment investment. The editor presents a business cycle model, using the parameters estimated by the authors of the various studies. While one may disagree with specific points and possibly the general framework, it is nevertheless true that a major benefit of a book like this is the common viewpoint adopted by the various authors. All too frequently quantitative studies of a particular subject proceed from diffuse points of view, and therefore the total effect has less impact than the sum of the parts. In the present case the unified approach has resulted in greater impact.

Several features are common to all the studies. First, all of them use the stock adjustment or "flexible" accelerator models, which have become increasingly exploited in econometric work for the study of durable goods. These models were first brought to the attention of economists by Richard Goodwin in 1948, and several years later by Hollis Chenery. A rugged parochialism, however, has led the authors to ignore these historical origins. The basic model has these characteristics: The net stock of capital demanded is presumed to be a constant proportion of the difference between the desired net stock at the end of the period and the capital stock at the beginning of the period. The proportionality constant represents "the speed of adjustment" coefficient. In these studies, the speed of adjustment varies between .05 and about .4 per year, depending upon the commodity. The demand for gross investment also includes a replacement component, where replacement demand is ordinarily presumed to be proportional to the stock of capital at the beginning of the period. The validity of the model, of course, depends upon what is included in "desired stock of capital" for the end of the period. The authors generally consider a highly conventional variety of economic variables, principally prices and income for consumer durables and a somewhat wider range of variables for producer durables. An outstanding feature of these studies is the serious consideration given to measurement of the stock of capital. Various essays, particularly the Griliches tractor study and the Chow automobile study, contain worthwhile and relevant discussions on the construction of empirical measures of real capital assets corresponding to pertinent theoretical economic constructions.

Second, the studies mostly depend upon aggregative time series and further,

to quote the editor, "all the studies rely exclusively, though not naively, on the least squares method of estimation." The statistical difficulties inherent in the use of aggregative time series are well known and need not be repeated here. Furthermore, cross sections would not be appropriate data for the estimation of the coefficients of principal interest to the authors of these studies. While these studies will and should provide a target and useful reference for much future work in the area of durable goods demand, subsequent advances in knowledge about the demand for durables is most likely to come from much more detailed consideration of the structure of the economy.

Third, all the studies use a weighted average of current and lagged disposable income, as an alternative to the usual Department of Commerce definition of current disposable income. The authors find that when the quantity of real capital is regressed against this smoothed value of income, multiple correlations are often higher than when the stock is regressed against current disposable income, along with the same set of other variables. This weighted average past income, attributed to Milton Friedman under the name of "expected income," is a useful device for measuring durable asset demand; after all, the stock of capital represents an accumulation of past investments (minus retirements) and hence is a relatively smoothly moving time series, so that it makes empirical sense to explain it by using "expected" income, a cumulant of past flows. In measuring flow demand, *i.e.*, the demand for investment, however, it was found several times that the current disposable income variant is a superior explanatory variable. In one interesting test of the Friedman "expected" income hypothesis, Gregory Chow defined "unexpected" income as the difference between Friedman's "expected" income and current disposable income as a separate variable, along with "expected" income. Both variables had about the same estimated coefficients. Hence, "expected" income turned out to be indistinguishable from current income in this particular application.

In the initial essay Arnold Harberger uses the estimated parameters to construct an aggregate cyclical model whose behavior is determined by the flexible accelerator for consumer durables, housing and plant and equipment. He assumes that in a typical recession, GNP falls to 4 per cent below "normal" in four quarters, and four quarters later resumes its normal level, while disposable income is assumed to fall below normal by only three-fifths of the drop in GNP because of the "built-in" stabilizers. Harberger concludes: "The picture thus emerges of a normal recession pattern in which the aggregate loss of product probably lies between 2 and 2.5 times the aggregate autonomous reduction in demand. The induced short fall of non-durables demand probably lies between one-third and one-half of the aggregate autonomous reduction, while the induced loss of durables demand probably amounts to between two-thirds and five-fourths of the autonomous reduction."

Here are some of the main findings of the individual studies: Richard Muth, in an analysis of the demand for residential housing, finds that both price and income elasticities of the demand for the housing stock are approximately unity, whereas the interest elasticity of demand for the stock of housing is quite low, about .15. This last result has some possible implications for mon-

etary policy. These implications, however, are limited by one major defect of this study. Muth chose an historical period during which the housing market was strikingly remote from the present by restricting the estimated models to the years 1915-1941. Government finance programs, such as FHA and VA had slight impact. In addition, most mortgages were not self-amortizing. Hence, the analysis consists of an economic history, which may have small structural significance for the present time. Furthermore, the lag adjustment structure, which is likely to be especially complicated in the case of residential construction, has been treated in a fairly cursory manner. Muth finds much higher price elasticities than those of two previous studies. His price elasticity ranges from .7 to 1.80, while previous studies showed price elasticities to be .5 or less. A firmly based estimate of these elasticities, however, must await a more complete specification of the structure than that proposed by Muth.

The demand for farm tractors in the United States has been explored by Zvi Griliches, who has used a particular variety of economic theory to derive a demand function for tractors. Thus he considers the demand for tractors to be a function of the existing stock of tractors, interest rates and the index of prices paid for tractors divided by the index of prices received for crops. We are informed that "this study, thus, differs from most consumer demand studies in the absence of a 'scale' variable like income. But this is quite consistent with theory. In the conventional theory of the firm, the firm has no 'budget restraint,' and the production function is the only constraint." Output is explicitly excluded, although implicitly it is taken into account in output prices. This formulation assumes that the firm is maximizing profits in perfect markets. Had the cost minimization approach been adopted instead, output would have appeared explicitly, as it does in the standard flexible accelerator models. To say the very least, I find it surprising to suppose that there are no capital market restrictions on farmers. For a great many years at least, farming has been carried on to a large extent by small-scale units whose access to the capital market has been restricted, so that purchases of tractors would then depend very heavily upon the income of farmers, as well as on the credit terms available from the tractor manufacturers. Closer attention to the institutional structure might thus have measurably improved the results, even though a measure of "proprietors' real equity in agriculture" was introduced to deal with capital rationing. This variable did not prove to be statistically significant. More likely, the deviations of desired from actual liquid assets, perhaps best represented by some function of farm income, determine the demand for tractors rather than proprietors' equity. Hence, I believe that Griliches may have some serious identification troubles. Suppose the elasticity of demand for agricultural products in the short run is less than unity, an assumption often made. Then high prices of farm products will be associated with low outputs and the additional, simultaneous relation introduced by this price-income relationship will also cause funds to be generated for farmers which will bias the estimated coefficients. However, one of the best available discussions in print on the problems of measuring capital stock has been written by Griliches.

Gregory Chow's study on automobiles is a re-evaluation of results to be found in his *The Demand for Automobiles in the United States*. He tests the degree to which a demand equation estimated with data ending in 1953 has predicted the demand for automobiles from 1954 to 1958. Chow found that the empirical equation forecasts well for the years 1954 through 1957, including 1955 which was the large model-change year. However, the recession year 1958 resulted in a substantial overestimate. Chow's study also is valuable because it presents earlier results on the different ways in which the stock of capital ought to be valued in order to get an empirically observable time series of the stock of capital. Chow found that the price of an automobile of a given model year declines with considerable regularity about 25 per cent per year so that he used relative price weights to get a value stock instead of simply adding up the total number of cars.

The investment study by Yehuda Grunfeld is the only disaggregated study. He fits time-series plant and equipment investment equations for nine large firms for the years 1935-1954, including the war years. While definitely suggestive and interesting, his results are not comparable with previous empirical results on plant and equipment investment behavior since he measures investment as the sum of maintenance and repair outlays plus gross dollar outlays on plant and equipment, instead of plant and equipment expenditure alone. Grunfeld finds that when a rough estimate of the replacement cost value of gross fixed assets *and* the market value of the firm (defined as the stock market evaluation of common stock on December 31 of the previous year, plus book value of debt) *and* corporate profits are included in the same regression equation, corporate profits are statistically insignificant. He concludes that whatever explanatory value profits have for investment in a simple regression equation, this simple correlation is a proxy effect most correctly represented by the value of the expectations of future revenues reflected in market value and the replacement value of fixed assets. The use of stock market prices in investment functions was first suggested by Tinbergen in his famous League of Nations *Statistical Testing of Business Cycle Theories* and was also given brief attention by Meyer and Kuh in *The Investment Decision*. While stock market prices undoubtedly deserve investigation as a measure of expectations, it seems puzzling that a variable so heavily subject to random disturbances and systematic forces additional to the particular fortunes of an individual firm should still prove such a powerful explainer of investment. After all, variations in stock prices originate from a great many things—shifts in liquidity preference, momentary panics associated with heart attacks of key government officials, or pronouncements of doom and gloom by prominent economists before Congressional committees. Grunfeld argues that systematic factors outside the individual firm ought to be considered. If correct, it is not obvious that the stock market is the most appropriate measure. This study contains some promising lines of analysis which certainly would have been further investigated had not the author most unfortunately met with a fatal accident.

The remaining study deserves briefer comment. Meyer Burstein's study of the demand for refrigerators, a skillful econometric job, ran into extremely

serious data difficulties. The price and income series were so highly correlated that obtaining reliable estimates of their parameters proved difficult. Burstein also grapples intelligently with the problems of taking into account quality changes, since refrigerators have been subject to large quality changes during the past three decades.

Subsequent studies in the area of durable goods behavior will benefit greatly from this collection. Every empirical study (theoretical too for that matter) is open to criticism: Who is to cast the first typewriter? This should not obscure the merit of these analyses, which rank two and one-sixteenth standard deviations above the average.

Edwin Kuh

Massachusetts Institute of Technology

The Classical Liberalism, Marxism, and the Twentieth Century. By Overton H. Taylor. Cambridge: Harvard University Press, 1960. Pp. ix, 119. $3.50.

This pleasant and urbane little volume contains four lectures given at the University of Virginia in 1958. They are reproduced substantially as delivered, and have a rotund oral flavor, even to the appearance of a philosopher named Hagel (p. 4) and Marxist utopia in which government would whither away (p. 70). In the first lecture we are introduced to classical liberalism as an ideal of America; in the second, we are introduced with the cool propriety of a perfect host to a somewhat dubious guest—the socialist and Marxian tradition. Then in the third lecture the two traditions, now in modern dress, confront each other on the great stage of the world, in a manner so civilized however as almost to suggest the Harvard Faculty Club. Finally in the fourth lecture some sage advice is tendered to Our Side, which has emerged a shade bloody but quite unbowed from the match of the third lecture, on how to improve its technique in the struggle for men's minds and hearts. The style is a rare pleasure; reminiscent a shade, perhaps a little surprisingly, or again perhaps not so in view of the long Harvard tradition, of Henry James in his later, or more parenthetical (not that this is any disgrace) period; or perhaps even, though this is less fashionable, of Carlyle; but it is, reminiscences or no, a pleasure in these days of short stilted sentences and niggling formulae to find an author who has the courage to clothe noble and sonorous ideas in an appropriate investiture.

Taylor stands in a great tradition of philosophical economists, and his work reminds one in many ways of the more recent works of J. M. Clark; one looks for insight rather than system, wisdom rather than techniques, and the reader will not find these lacking. To give but a small sample: the Thomistic roots of Lockean liberalism and the sharp conflict of both with Machiavellianism and mercantilism is beautifully displayed in lecture I: the historical character of liberalism by contrast with the historicism of Marxism is strikingly brought out in lecture II; the discussion of the "moral use of reason," the epistemological basis of "con-science," the contrast between liberal and ethical democracy with absolute and mechanical democracy in the two final chapters are likewise beautifully done. Nobody, I suspect, will be much startled by this

book; nevertheless it is a book to be enjoyed and savored even by those who are familiar with the ideas which it expresses. If the great questions receive somewhat diffident answers, as Taylor himself acknowledges with engaging modesty, perhaps this is the kind of answer that great questions ought to get.

K. E. BOULDING

The University of Michigan

Early American Policy: Six Columbia Contributors. By JOSEPH DORFMAN and R. G. TUGWELL. New York: Columbia University Press, 1960. Pp. 356. $6.00.

This attractively produced volume is, strictly speaking, a work of supererogation, for its two eminent and prolific authors earned a place in the scholar's Elysium long ago. Moreover, of the six essays, five were originally published in the *Columbia University Quarterly* between 1931 and 1938, and severely condensed versions appeared in the first two volumes of Dorfman's *The Economic Mind in American Civilization.* The sixth, dealing with John Jay, is new.

While the underlying motive is pietistic, the connecting link between the essays is, appropriately enough, institutional. Each of the contributors referred to in the title was, at some stage in his career, connected with Columbia College or its predecessor, King's. Alexander Hamilton, John Jay, Henry Vethake, John McVickar and William Beach Lawrence were all students—though Hamilton, whose period of instruction ended with a "shower of stones," did not graduate. (Incidentally, Vethake's experience of academic life was even more dramatic. On one occasion, while prostrate from an attack by a student, he was rescued by the chemistry professor who was armed with a pair of tongs!) Lawrence (briefly), McVickar and Francis Lieber were faculty members. Beyond this institutional link however, they had little in common, for their contributions to policy were as varied as their careers. Indeed this book affords ample testimony to the promiscuous character of the term "policy." Only Hamilton and Jay, both of whom were statesmen and hence policy-makers, had any direct influence on national policy. McVickar, Vethake, and Lieber spent most of their lives as scholars or educational administrators, and although they often wrote on policy questions and occasionally participated in policy debates in an era when the dividing line between scholarship and propaganda was much fainter than it is at present, they rarely exercised any important influence on policy-makers, and cannot be said to have played a leading role in the formation of public opinion on current issues.

Employing a pleasantly informal style, and proceeding at a leisurely pace, the authors provide a wealth of interesting and scholarly detail on each personality. There are fascinating sidelights on early American academic life and, of course, authoritative accounts of the economic ideas. Each chapter includes a character sketch, a discussion of the man's beliefs and career, and an assessment of his contribution against a broad historical background. Although the reader may occasionally feel that the expenditure of scholarly resources has been prodigal, the authors have generally avoided the mistake of overestimating either the importance of their subjects or the influence of Co-

lumbia. The present essays (apart from the one on Hamilton) differ either in form or content from the originals, and in some cases significantly. The three appendices include a valuable short treatise on bills of exchange by McVickar, some letters from Lawrence to President Buchanan, and some specimens of Francis Lieber's examination questions.

A. W. Coats

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The Economic Point of View: An Essay in the History of Economic Thought. By Israel M. Kirzner. Princeton: D. Van Nostrand, 1960. Pp. xv, 228. $5.50.

What *is* economics? It is not a jesting Pilate question. Every economist at some time must have wondered just what it was that he was doing. The fortunate discover the definition that economics is what economics does, and resume what they were doing before it occurred to them to wonder what it was.

It is not everyone's answer. Some want to know just what it is that makes a fact, an idea, or a problem a part of economics. What for example is there about the content of the books reviewed on these pages which puts them within economics? The only book which indisputably belongs is one that tries to answer these questions, as this book does. It is about a real as distinct from a nominal definition of economics, and that is something meant to "reveal to us the 'nature' of the *definiendum*—which in this case is a concept, a 'point of view'."

Professor Kirzner explains the idea historically. We are told about those writers who believed economics was the science of wealth or of welfare, a point of view shared (he states) by Smith, the Ricardians, Marx, the Austrians, the Lausanne school, and Wesley Mitchell—a crowded gallery. Another viewpoint is that economics is the study of behavior motivated by pecuniary self-interest or the wish to maximize some kind of returns (the Ricardians again, Jevons, and Viner). Another is that economics comprehends behavior in which money is important in one way or another, as Pigou believed it was the measure of the thing maximized. Today most economists believe that what they are doing is studying how men behave when they must relate unlimited ends and scarce means. The idea is attributed to Lionel Robbins, but F. H. Knight has stated it more cogently. At the end of the historical survey we come to the praxiology of Ludwig von Mises, by which economics is defined as the science of human action and all human action as purposive or rational behavoir. The idea is expressed with deep conviction and with dedication to its originator who was the author's teacher.

But it is not convincing. If all behavior is rational, then rationality means so many things that it cannot be used to predict behavior. That is a methodological objection. The definitional objection is just as strong. If economics is the study of all purposive behavior, the economist should study all behavior. But that cannot be, and Kirzner acknowledges this with commendable frankness. Still he believes that economics has a "nature of its own," although he does not say what it is. In the end we are told that any distinction between eco-

nomics and other studies of behavoir "must be arbitrary." That is a disappointing end for a book which means to reveal to us "the 'nature' of the *definiendum.*" At this point we are prepared to accept the jesting definition that economics is what we do.

The definition is not in this book, which like others on the scope and method of a subject is serious and often solemn. It obviously has been prepared with much thought, the historical sections are responsible and as detailed as possible, and the writing is as clear as the subject allows it to be. But the purpose has not been achieved, and that, I think, is not a failure of the author but of the leading idea with which he worked.

WILLIAM D. GRAMPP

University of Illinois in Chicago

Economic History; Economic Development; National Economies

Six Lectures on Economic Growth. By SIMON KUZNETS. Glencoe, Ill.: The Free Press, 1959. Pp. 122. $3.50.

Professor Kuznets' published lectures on economic growth, originally presented in Mexico City in 1958, are certainly a welcome addition to the literature in the field. There are, of course, a number of books that use some statistical data, but there is probably no other book that summarizes so much of the statistical findings in so short and readable a format by one of the most respected authorities in the field. Many of the statistical generalizations asserted are likely to come as no surprise to students of economic development, but they are, nevertheless, of interest to us since it is Kuznets that asserts them. Had someone else tried to make some of the points we might put less credence in them.

Lecture 1 reviews "modern" rates of growth, and points to the fact that from a long-run point of view, modern growth rates are especially high. Indeed, Kuznets makes much of the point that they could not have persisted for a very long time, because if we extrapolate the current high rates of growth backwards we find that they must have been of recent origin. This argument is in fact probably true, but it need not be necessarily so if in the past there had been large long-run cyclical variations in growth rates and output. This is, of course, a minor logical quibble and should not be taken seriously.

In lecture 2 Kuznets considers the time pattern of economic growth and emphasizes that technological change is the necessary condition and the major factor in modern growth. Throughout the book Kuznets uses the phrase "modern economic growth" in connection with the high growth rates achieved in modern times. But one also gets the impression that by modern economic growth one should have in mind the type of growth that is really based on industrialization and mechanization. Surely it is industrialization and utilization of newly discovered sources and means of harnessing nonhuman power and its potentiality that distinguishes modern from ancient growth rather than the rates of growth themselves. The average rates of growth are, of course, very different today than they were in the distant past. But this is the consequence rather than the causal aspect.

Lecture 3 will be familiar to students of economic development who have attempted to grapple with the statistics and generalizations presented by Colin Clark and the counterarguments presented by Bauer and Yamey. The matter discussed here is the uniformities to be found in the changes in the industrial structure of the labor force and in the national product as a country develops.

Lecture 4 considers the long-term trends in capital formation for a number of countries. Kuznets finds, for instance, "that the ratio of gross domestic capital formation to gross domestic production is positively associated with income per capita" (p. 71). Another point that Kuznets makes is with respect to the differences in net capital formation as a percentage of net national income between advanced and underdeveloped countries. He finds, for example, that "the share of net capital formation in net national production in groups I and II [developed countries] exceeds that in group VII [underdeveloped countries] by 8% of national product" (p. 74). Is it plausible to assume that these few additional percentage points account for the striking differences in the level of performance and patterns of growth that we observe between the developed and underdeveloped countries? He concludes that he has a strong intuitive objection to such a theory of history since it places strategic importance on minor details. There is the probability that the ratios of growth that exist today are probably symptomatic of the recent situation and do not reflect a condition that goes back very far in history.

Lecture 5 considers the problem of size. In this area Kuznets seems to conclude that for the most part limitation of size is not really an important handicap to development.

Lecture 6 is of special interest to the economic theorist. In this lecture Kuznets prescribes some tasks for economic theory. The tasks that he sets seem to be formidable indeed: (1) to develop a testable model that would weave the common (and presumably statistically determined) elements of the industrial system, the structure of common ends and aspirations, and finally the cultural and social elements of such economies into a unified whole; (2) to formalize the effects of different initial conditions on economic growth; and (3) to develop a theory as to how growth spreads from one area to another or from one country to another.

This brief review of the contents cannot really give the feel of the book. It seems to me that Kuznets is out to determine two things on the basis of statistical evidence: First, what are the common elements and tendencies that we find in different countries in the course of economic growth? Second, what are the differences that we see? Kuznets discusses both simultaneously although he seems to have a slight bias for underscoring the differences. What generalizations can you make about economic growth if you become immersed in the facts? For facts we should really substitute the word statistics, for these are for the most part, the facts that are emphasized. Kuznets seems to insist, without saying so directly, that we stick to the known statistical facts before we speak.

Despite his high degree of caution, Kuznets does find a number of common

patterns that evolve in the course of growth. Surely this is a hopeful sign. For it means that we may be able to develop some theoretical structure consistent with common elements. At the same time, one is rather surprised that rarely in the volume does Kuznets discuss the reliability of his statistics. What stock can we put in any of these generalizations based on statistics, be they generalizations about common tendencies or differences, if nothing is said about the degree of reliability and the range of probable error. I, for one, would have been intrigued had Kuznets ventured into this area. This last does not by any means reduce substantially the basic value of the book. Though many of us may have believed that there are certain common tendencies to be observed in the course of development, it is surely helpful to get the weighty authority of Kuznets behind such beliefs.

On the tasks for economic theory I suspect that Kuznets may be placing the theorist in a bit too much of a strait jacket. His order seems a rather tall one. Certainly we want theories that fit some of the facts, but must they fit primarily the statistical facts stressed here as well as other social, cultural and institutional facts simultaneously? Do not the facts that we want to select and stress depend in part on their nature and in part on their reliability?

What is really important is to be able to develop theories which are not tautological. They must be consistent with some of the facts of the real world. Economics is, after all, an empirical science. But in the early stages—and surely we are in the early stages—would it not be better to leave the theorist enough leeway to pick for himself the facts that he wishes to tie together within his theory. In other words, the order that Kuznets places for theory seems to be an order for an optimal theory in some sense. Such a request is probably not too helpful at this juncture. Rather, should we not grapple with the problem of trying to determine the present *feasible* theory set and request, at most, the optimal one out of the feasible set?

HARVEY LEIBENSTEIN

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Iniziativa privata e azione pubblica nei piani di sviluppo economico. By PASQUALE SARACENO. No. 1 of Monograph Series SVIMEZ (Association for the Industrial Development of Southern Italy). Rome: Giuffre, 1959. Pp. 101. L. 600.

Professor Saraceno, one of Italy's leading economists and director of economic studies of SVIMEZ, has prepared this monograph from lectures delivered in 1957 at the Universities of Ankara and Istanbul and at the International Bank for Reconstruction and Development. He writes as an expert in the problems of unification and development of the Italian economy.

His basic theme is that the state in an underdeveloped economy acquires major tasks not forseen in western politico-economic thinking. The Keynesian approach is inadequate because it emphasizes the promotion of effective demand, letting the market rule from there on. Western socialism looks to destruction of the private sector, has its eyes fixed upon income redistribution, and wholly overlooks the problems of capital formation and income growth.

If, as Saraceno passionately desires, an underdeveloped country is to industrialize without recourse to communist totalitarianism, then it must combine an over-all development plan with a mixture of public and private firms. The public firms must function within a market order. If the tasks laid on them compel losses, these must be explicit so as not to mask inefficiency. As development proceeds, public firms may pass into private hands. Thus Saraceno contemplates a mixed economy, with framework and directive planning to effect the transformation to an industrial order. Old ideologies simply will not do for guides.

Saraceno points out that the advanced capitalistic countries had long ago abandoned automatic mechanisms. Keynesian policies are simply the last in a long chain. Very persuasively he argues, as Myrdal does, that capitalistic industrialism never encompassed the world save in limited exchange relations. Thus it achieved unified economies only in selected areas, while the remainder failed to develop. The choice lay between intensified development at home, plus an eventual welfare state, or much greater investment abroad, to develop a balanced world economy. The former path was chosen. An unbalanced economic world was the result. For the undeveloped and underdeveloped portions market forces and private initiative will not do the job. The state must deliberately undertake the task of transformation.

Here the choice lies between communist methods and a new kind of liberal state, one that uses the market and private initiative where possible, encouraging their growth within an over-all plan. The purpose is to create a unified market economy, where the forces of initiative themselves are too weak. This means the reorganization of peasant agriculture, with absorption of displaced labor by surrounding sectors. Because much labor is unsuitable for modern techniques, there will be a kind of labor shortage, dictating use of capital-intensive methods in industry. As the industrial sector grows, a dual economy will emerge—in productivity, incomes, and growth rates. If industrial wages are tied to productivity, undesirable inequality of incomes and excessive consumption will occur. Wages and monopoly prices must both be controlled, to check the splitting of the economy and the population. The over-all allocation of savings and investment must be fitted to the plan, and deficit financing must be avoided. The state must both fill in for and supplement private enterprise. Once growth becomes self-sustaining and the economy is well unified, private initiative can expand more readily.

All this is familiar, but it needs to be said again and again. The gospel of "leaving it to the market" will not work, and has not worked, in the countries with which Saraceno is concerned. The bourgeoisie are simply not there in the first place. If these nations are to be saved for the free world, they must follow different political precepts. If they take Saraceno's road, they can get a unified market economy and freedom together. The alternatives are continued stagnation or centralized communist control, with the latter the untoward outcome of the former.

George H. Hildebrand

Cornell University

Zur Theorie der Industrialisierung. By SIGURD KLATT. Cologne and Opladen: Westdeutscher Verlag, 1959. Pp. 546. DM 45.—.

In this book we have before us an attempt to present a comprehensive theory of industrialization, including not only the economic, but also the important noneconomic aspects of this process. The result of this effort may be summarized in two main points: (1) Klatt does not make a new theoretical contribution; on the contrary, his presentation is a restatement of previous theories of economic growth, mostly in a form elaborated in the pertinent United States literature. More concretely, Klatt restates the theory of growth of E. D. Domar and Roy Harrod modified by D. Hamberg, R. Eisner, W. Fellner, and others. Hence, on the purely theoretical side, this book will not be of interest to an American reader familiar with the literature on economic growth theory of the last few years. (2) But if this book, though competent, is unoriginal on the purely theoretical side it does present a rather novel and, as I hope to show, useful organization of the material pertaining to the theoretical analysis of the industrialization process. To begin with, Klatt apparently has seen and has drawn upon a vast amount of literature, much of it in English. He appends a classified bibliography which covers 80 pages, and which is a valuable ingredient of the book. But more importantly, he makes the process of industrialization, its general theoretical analysis and its concrete manifestations in given historical situations, eminently intelligible by the very orderly arrangement of the subject matter.

The structure of the book's contents, although related to a stages-of-growth approach, constitutes a conversion of stages into analytical categories associated with economic development. More concretely, the book is divided into three main parts: the first deals with the initiation of the growth process, *i.e.*, the problem of identifying the factors which lead to the rapid stepping-up of economic growth; the second—and largest—part is devoted to the analysis of the industrialization process itself; and the third relates to the analysis of the "postgrowth stagnation," *i.e.*, a flattening out of the growth curve after a period of acceleration (which it has become fashionable to designate as "take-off" but which we may also call by the more time-honored term "industrial revolution").

It is clear from this brief sketch of the contents of Klatt's book that the organization of his material has some resemblance to the description of the growth process by W. W. Rostow. But the similarity is chiefly superficial and based on the assumption underlying both Rostow's and Klatt's analyses that the industrialization process, if plotted diagrammatically, could be represented as a line resembling a logistic curve. The most interesting part of Klatt's book deals with the explanation of the sudden upturn of the curve from its early rather flat to its steep section. This section of the curve corresponds to the period of the elaboration of the preconditions of industrialization and the actual industrial revolution. Instead of using imaginative speculation, Klatt approaches the explanation of the take-off with his customary propensity to synthesize.

First he establishes that there must be present a social, legal, political, and

general environmental framework in which industrial growth can take place. Then he turns to the actual "impulses" to industrialization and shows that various conditions—growth of population, change in productive technology, alteration in the structure of demand, and others—may create conditions conducive to economic growth. Here Klatt does not go far enough. Other "impulses" have been noted by other students of the growth process which, in some cases, have had profound significance. Among these the two which have received the greatest attention in recent literature are the stepping up of the national savings ratio (W. W. Rostow) and the importance of export markets (A. Youngson). Finally Klatt integrates the set of variables he has selected as partial "impulses" by bringing in the Schumpeterian theory of innovation and by stressing the role of entrepreneurship. This leads him to investigate also some psychological dimensions of the display of entrepreneurship, but here his performance is perfunctory and disappointing.

The general impression gained from this book is that it has an eminent didactic value due to its synthetic quality. It would constitute a good text for a senior or beginning graduate course in economic development. Since much of the literature absorbed and utilized in this book is in English, and may be assumed to be unknown to many German students, it is certain to have a wholesome impact on the study of economic growth in Germany. For us it may serve as a model of how an immense body of widely scattered literature can be summarized with advantage, and how the best parts of this literature can be used as material for a comprehensive account of a subject whose empirical manifestations are of the greatest variety and diversity.

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Ekonomicheskoye razvitiye Sredney Azii. (The Economic Development of Central Asia.) By Alim Muminovich Aminov. Tashkent: Academy of Sciences of the Uzbek SSR, 1959. Pp. 298.

The subtitle on the cover indicates that the book is devoted to the "colonial period" in the history of economic development of Central Asia. According to the author, this encompasses the period from the 1860's to 1917.

The attempt to present "colonial exploitation" as a historically benevolent phenomenon when Russia was the exploiting power is not novel with Soviet economic historians. The author is a respected student of the economic history of Central Asia. He is a native of Uzbekistan and is a product of the educational advance of this area during the Soviet period. He states his position with regard to the "objectively-progressive significance of the incorporation of Central Asia into Russia" as follows:

> . . . The incorporation of Central Asia into Russia in spite of the colonial oppression *objectively* [italics mine] met the requirements of the vital interests of the Uzbek nation and has saved it from the likelihood of English enslavement, in which some nations of Asia and Africa currently find themselves.
>
> The incorporation into Russia has created possibilities for the develop-

> ment in Central Asia of more modern forms of economic and social relations. It has resulted in serious changes in the economy, the social order, the development of capitalistic relations; [it has imposed] substantial limitations upon the patriarchal-feudal relations; [has furthered the] specialization of the region in the output of a marketable product—cotton—and the creation *eo ipso* in Central Asia of a cotton region, the involvement of the region in the world's commercial-monetary circulation. (Pp. 70-71.)

Apart from the declarative statements which proscribe the general framework of analysis, the book by Aminov is valuable because of its attempt to provide a factual account of the development of this particular region and its interrelation with the Russian economy. The author describes and analyzes the economy of the Central Asian states during the first half of the nineteenth century, the organization of agricultural production with special emphasis upon the conditions of land tenure, the system of taxation and the pattern of foreign trade, which reflected the growing dependence upon the Russian market. This description is useful as a background against which the policies and changes introduced by the Russians during the subsequent period stand out more vividly.

A very detailed exposition of the economic policies applied by the local and central authorities after the Russian conquest follows. The author incisively points out the shifts and frequent inconsistencies in the policies prior to the decision to convert Central Asia into a raw-material supply base for the Russian textile industry. The growth of the cotton economy of Russian Central Asia is reflected in the expansion of the area sown under cotton during a quarter-of-a-century as follows:

COTTON SOWN AREA IN TURKESTAN FOR SELECTED YEARS (IN HA.)

	1888	1890	1913	1915
Fergana Province	37,876	55,872	295,867	367,654
Syr-Daria Province	28,231	25,674	83,823	80,900
Samarkand Province	8,718	18,953	35,292	60,714
Transcaspian Province		983	46,845	62,780
Total Turkestan	74,825	101,481	461,828	572,048

Source: V. I. Iuferev, *Khlopkovodstvo v Turkestane*, Leningrad 1925, p. 136.

However, a topic which interests this reviewer, and which was dealt with only in passing by the author, is the market response of Central Asian agricultural producers, especially to cotton prices and to the introduction of new cotton varieties.

The process of adaptation of new varieties (American upland) and the substitution of upland cotton for local varieties was very rapid indeed. Within the five year period 1883-1887 the area under upland cotton increased from 36 ha. to 17,113 ha.; and by 1890 in the cotton areas of Turkestan (Russian administrative description of Central Asia except the Khanates of Bukhara and Khiva) 64,305 ha. of American upland was planted vs. 34,915 ha. of local varieties. By 1900 only about 5 per cent of the total cotton area was still

planted with local varieties.[1] This process took place under the impact of the following conditions and policy measures: (1) the existence of a price differential between upland and local cotton varieties; (2) the higher yield of upland cotton in most areas; (3) a lower rate of taxation for land occupied by upland varieties of cotton; (4) the extension of the railroads to Central Asia, so that cotton could be more cheaply exported from and grain imported into Central Asia; (5) a general increase in tariffs on cotton.

Another interesting phenomenon was the sensitivity of cotton acreage and output to grain prices. The flexibility of cotton producers of this area in shifting between cotton and grains was exhibited on at least two occasions. Cotton acreage decreased in 1902-3 when high grain prices coincided with low cotton yields in Central Asia and low cotton prices on the world market. Cotton acreage also sharply declined during the years following 1916 when grain prices increased steadily and the cotton prices, controlled by the State, were kept stable. (Aminov, p. 216).

A problem of some interest to students of agriculture in backward economies, which is neglected by the author, is the relative success or failure of plantations vs. small-holding farms. All attempts to develop cotton plantations failed in Central Asia during the prerevolutionary period. The explanations advanced by the author are too general and indicate that his research in this problem area was probably insufficient.

Also the problem of credit facilities and their availability to the cotton producers is handled by the author in a pedestrian way. He describes them as combinations of the well-known money-lender type of operation with similar practices followed by the banks and credit institutions. It is true that the bulk of the bank credits were extended to large-scale operating middlemen and to procuring textile firms. However, the author has failed to mention that by 1909 the government embarked on a policy of establishing new forms of credit for small-holders. Although one can point out that the 482 credit and savings and loan associations which extended credit to about 112,000 cotton producers in 1914 were able to meet only a small fraction of the demand for credit of the cotton producers, there is no excuse for not taking note of the interesting institutional development and its role.

One of the virtues of Aminov's study is the availability of an extensive bibliography, which is seldom encountered in Soviet publications. The bibliography includes references to archive-documents, periodicals, and monographs from the prerevolutionary as well as Soviet periods.

Students of economic development in general, and of the Central Asian area in particular, will find valuable data and materials in Aminov's study.

ARCADIUS KAHAN

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The Hungarian Experience in Economic Planning. By BELA A. BALASSA. New Haven: Yale University Press, 1959. Pp. xii, 283. $6.00.

Mr. Balassa's book is an interesting and worth-while discussion of Hungar-

[1] Increase of area under upland cotton 1883-1887, from Aminov, p. 149. For the 1890 relationship, Department of Agriculture, Ministry of Crown Domains, *The Industries of Russia*, Vol. III. *Agriculture and Forestry*, St. Petersburg 1893, p. 144.

ian business administration in 1949-56. Despite its title, however, it is not really a study of economic planning.

The core of the book is a discussion of management in industry and construction. In it two basic problems emerge. In the first place, the central goverment agencies found it extremely difficult to prepare directives to plant officials. As a result, the plants operated, for practical purposes, on the basis of "quarterly plans," which amounted to little more than *ad hoc* directives. In the second place, directives were formulated mainly in terms of quantities of output and allocations, with price policy obviously of negligible importance. Therefore, planning followed the procedure of "materials balancing" or, as Balassa puts it, partial equilibrium input-output. It appears to have been excessively difficult to place numercial values on the coefficients of these systems: general equilibrium problems intruded themselves, plant performance varied widely, and so on. One is led to wonder whether the economy may in fact have been nonlinear in important respects. In this discussion Balassa stresses the cost of obtaining information and of supervising centralized directives.

This part of the discussion will not surprise the student of the USSR. He will be at home among the institutions and directives (although Balassa does not point this out), and will recognize as well the violators of orders. In this sense, the book is of interest to all students of Eastern Europe.

Again, Balassa is struck by the disparity between ministerial and plant functions, and is led to speculate on the theory of the firm. Here he is following in the footsteps of those Sovietologists who suspect that even monolithic economies need a microeconomic theory. Balassa will not delight the contemporary formalist school, but he takes a useful step of a more impressionistic sort.

He points out that Hungary underwent reconstruction (1945-47), a three-year plan (1947-49), accelerated industrialization (1950-53), a New Course (1953-54), renewed industrialization (1955-56) and revolution (1956). He states (p. 43) that "The rapid turns in economic policy . . . can be characterized as being based largely on political deliberations, while economic considerations were greatly neglected. . . ." If so, one might wonder whether the administrative problems he describes might not simply reflect a grasshopper economic policy, and whether any further conclusions can be reached from the data. If, however, in some sense management policy did show continuity, it would have been interesting to consider what changes in economic policy would necessarily entail changes in the policy toward plant management.

But these problems, however interesting, do not exhaust economic planning, even in an economy such as the Hungarian. The government, through its budget, controls virtually all construction, and the banking system influences inventories and the money supply of enterprises and consumers. Balassa virtually neglects the budget, regarding investment solely in terms of the management of construction enterprises; he rules out the banking system (p. v) as having nothing to do with planning; and while he concedes briefly (pp. 118-19) that prices have a macroeconomic aspect, in fact he deals only with prices of individual commodities. His discussion of the consumer sector deals basically with the standard of living (here he is interesting, even if his

discussion is on quite a different level from that in the rest of the book), and one would not guess that what went on in the consumer sector had any impact on either enterprises or the government.

Finally, I have found no mention of the fact that the USSR was co-owner of much of Hungarian industry over much of the period under study. If this fact was of no importance in Hungarian economic planning, those who speak of "Soviet colonialism" should be so informed. If it did matter, Balassa should have indicated how Soviet interests in the respective properties appeared throughout the managerial process.

EDWARD AMES

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Inflation in an Underdeveloped Economy: A Study of Inflation in India. By SANTIKUMAR GHOSH. Calcutta: World Press Private Ltd., 1959. Pp. xiii, 179, 39. Rs 11.—.

The first quarter of the book evolves a theory of inflation applicable to underdeveloped economies. At the outset the author minimizes the usefulness of the inflationary-gap approach, turning rather to a monetary explanation of inflation. Underdeveloped economies continually operate at a bottleneck level comparable to full employment in an advanced economy. Although labor may be redundant, capital scarcity prevents expansion in response to increased demand. Under these circumstances monetary theory comes into its own in explaining price variations.

He points out that the usual instruments of monetary policy may be of limited use in an economy where banking is little developed, but control of the volume of currency gains in importance. To the extent that control of credit is effective in combatting inflation, he argues that it must include specific and direct controls.

Returning to income analysis, he shows the limited usefulness of fiscal policy in an underdeveloped economy. Governmental expenditures for development are essential whatever their cost, but taxation is limited by the poverty of the people, particularly if the government has humanitarian motives. Some inflationary pressure from deficit financing must therefore be expected. He argues that the private sector should bear the burden of having its investment and consumption reduced sufficiently, by direct controls, to prevent serious inflation. He almost certainly underestimates both the capacity and the need to increase taxes, even in poor countries, which would reduce inflationary pressures with less need for the whole array of direct controls he envisages.

The major portion of the book is an informed and careful survey of inflation in India during the war, the immediate postwar years, and the more recent period of development planning. During the war wholesale prices rose by 144 per cent, most of this rise coming between 1942 and 1944; this was an inflation caused primarily by large purchases for export or for use by Allied troops in India. In the early postwar years the import surplus helped to damp down inflationary pressures. During the first five-year plan, 1951-56, the import surplus, financed in considerable part by blocked sterling balances accu-

mulated during the war, offset government deficits so that prices dropped (with a generous assist from two very good harvests). The argument of this section is well balanced, thorough and careful; he marshals the facts well to support each point. It is a more detailed study of Indian inflation than any other this reviewer has seen. The book will be of great value to those who want a detailed study of Indian inflation, or to those who seek a survey of monetary and income theory as it applies to inflationary problems of an underdeveloped area. The organization of the theoretical section is lucid and concise, but the presentation of the Indian material is disorganized and repetitive.

The appendix develops a statistical measure of inflationary pressure built on *ex post* data for private investment, government deficit and the export surplus. If the sum of these quantities rises relative to national product it is taken as an indication of inflationary pressures. Obviously this is true only if saving has not likewise increased relative to income, so the author assumes that planned saving is a constant proportion of income. While this assumption may have been valid for the period of wartime controls, it hardly seems warranted for the postwar period. In addition his measure suffers from an error of detail. It moves rapidly in the downward (wrong) direction during the worst part of the war inflation, and thus is positively misleading. The error is that the government deficit figure omits the large wartime supplies purchased by the government for Allied armies. These deficit-financed purchases were no less inflationary because they were handled outside the budget; he correctly discusses this problem in the text, but omits consideration of it in his statistical appendix with the result that his measure moves perversely. He fails to define exactly the components of this measure, and only by looking to the original sources of the data can one discover the exact definitions used. Aside from this laxness, the study is a very informative one.

WILLIS D. WEATHERFORD

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East and West in India's Development. By WILFRED MALENBAUM. Washington: National Planning Association, 1959. Pp. xi, 67. $1.75.

This concise monograph, which was published in the series on "The Economics of Competitive Coexistence," deals with a very important subject. It reviews critically India's five-year plans, its prospects for economic growth, and discusses aid to India from East and West. In the section on "The Challenge to the West" the important point is the one emphasized in the introductory statement by the Special Project Committee on the Economics of Competitive Existence: "Thus while raising the West's financial stake, thought might also be given to co-operation with the Indian leaders with a view to achieving not only numerical targets, but also to recapturing the broad intent of the Plan in a search for a development pattern consistent with Indian potential," (*i.e.*, less accent on heavy industry in the public sector, more emphasis on agriculture, small industry and the private sector in general).

The main subject of discussion is India's second five-year plan (1956-61), its size, its investment pattern and the greatly increased role of the public

sector. The author does not agree with the plan's emphasis on heavy industry and the expansion of the public sector in this field. He rightly points out that the plan underrated the importance of expansion in lighter industries and consumer goods production. Apparently, however, the author does not appreciate sufficiently the foreign exchange difficulties which have resulted from the investment pattern of the plan. India embarked upon development with unbalanced growth without the consequences, particularly for the balance of payments, having been properly examined. The foreign exchange requirements of the plan were greatly underestimated—the author speaks of the Planning Commission's optimism in its calculations of costs. As early as the second year of the plan it became known that the balance-of-payments deficit would greatly exceed the plan's estimate of $2.3 billion; it will turn out to be nearly $4 billion.

Because of the heavy drain on foreign assets strict import restrictions were imposed late in 1957. The decline in foreign assets was the result not only of substantial imports of capital goods in the private sector—which the author overrates—but also increased imports for investment in the public sector. Moreover, there were large general imports, which had been allowed in the wave of optimism left over from the comfortable balance-of-payments position during the first plan; and there also were large defense imports.

During 1958, the government had to make a decision to reduce the plan to the so-called "core" in order to reduce the drain on foreign assets (they declined by more than $1 billion during the plan period, 1956 to 1961). However, any major change in the plan was limited by the large commitments incurred abroad, mainly by the public sector. The "core" included most of the important projects. Malenbaum is too pessimistic with regard to the effects of the reduction of the plan in the public sector, which in money terms will be about 4 per cent, or much less than the reduction in the private sector.

It is now estimated that 80 to 85 per cent of the physical targets of the plan will be attained and national income is expected to increase by about 20 per cent over the second plan period—which should be regarded as a good performance.

The chapter on the contrasting roles of foreign assistance is very informative in showing the coexistence of aid from West and East in India. Although the communist countries' assistance, which is concentrated prevailingly on heavy industry and oil, is more conspicuous than the assistance from democratic nations, which is spread over many fields of the Indian economy, this reviewer believes that the effect of this difference on Indian public opinion should not be exaggerated. Incidentally, Communist aid amounted to only about 18 per cent of total foreign aid, not including the U.S. substantial supplies of food grains under public law 480.

Writing before the third plan was formulated, Malenbaum suggests that greater emphasis should be given to labor-intensive activity, that government investment be focused more on specific bottlenecks, and a larger role be given to the private sector. He points out that such a shift would conform better to the actual pattern of savings and investment flows, and would mean a closer

approximation to the traditional role of the Indian government in economic matters. This reviewer agrees in principle with these views.

However, the outline of the third plan while aiming at self-sufficiency in foodgrains places strong emphasis on heavy and machine-building industry, in order that it may be possible to meet the requirements for further industrialization within ten years or so, mainly from the country's own resources. Of the total proposed investment of $21 billion, $13 billion has been assigned to the public sector. The plan assumes that the total foreign exchange requirements of more than $6 billion will be provided from external aid. Sizeable credits have already been obtained from the Soviet bloc and large supplies of foodgrains are assured from the United States under public law 480.

Should India be unable to obtain very substantial assistance, the size of the plan will have to be reduced. The Western powers are faced with a very important decision, namely how much aid to provide for India in order to enable it to continue its development.

Space does not permit a review of various other subjects considered in this study, which is packed with ideas and information and which should stimulate further discussion of this vital problem.

ANTONIN BASCH

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Impact Multipliers and the Dynamic Properties of the Klein-Goldberger Model. By ARTHUR S. GOLDBERGER. Amsterdam: North-Holland Publishing Co., 1959. Pp. 138. $4.00.

This thorough and painstaking book fills a gap in the presentation of the noteworthy Klein-Goldberger econometric model of the United States.[1] It uncovers many of the economic characteristics of that model which had remained implicit in previous discussions.

After briefly reviewing the original Klein-Goldberger econometric model, the author derives and evaluates the "impact multipliers" of the model. These are the coefficients which describe the total response of the endogenous variables in the initial period to a unit change in one of the predetermined variables. In succeeding chapters the author analyzes the dynamic responses of the endogenous variables of the Klein-Goldberger model over short and long periods. The period-by-period predictive ability of the model is also investigated for the years 1929-1941 and 1946-1952.

This book can serve for practitioners of econometric forecasting as an example of the analysis necessary to illustrate the full implications of an econometric system and to obtain from it all the interesting results. For example, it is no surprise to learn that "impact" multipliers are less than final or "equilibrium" multipliers. However, it is interesting and useful to learn that the impact multiplier of government expenditures on GNP, with the given tax rates, is 1.23 while the final multiplier is 2.34. Similarly the other numerical

[1] L. R. Klein and A. S. Goldberger, *An Econometric Model of the United States, 1929-1952*, Amsterdam 1955.

estimates of multipliers add to our factual knowledge of these significant coefficients.

One of the interesting results of the author's analysis is that the part of the model which deals with real variables is only loosely related to the price-explanatory part. It was also found, to paraphrase the result, that the model was larger and more complicated than it needed to be for the results obtained. These features of the model simplified its subsequent analysis considerably. They also contribute to the conclusion that much more investigation of the basic behavioral relations of econometric models is necessary. The author shows a full awareness of this conclusion.

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Die sowjetische Wirtschaftspolitik am Scheidewege. By ERIK BOETTCHER. Tübingen: J. C. B. Mohr (Paul Siebeck), 1959. Pp. xvi, 307. DM 26.50.

This book falls into two basic parts: the first deals with the consequences deemed to flow from stringencies in urban labor supply; the second discusses the role of ideology in the formulation of Soviet economic policy. The first four lengthy chapters examine the elaboration of the planning idea, changing labor organization, the bottleneck in agricultural production, and the rationalization and "decentralization" of industry. The fifth and last chapter discusses ideology and economic policy.

The introductory chapter presents only in a cursory manner the underlying assumptions and the operation of planning. The problem of the interrelations between national, sector, and enterprise plan and between elaboration, implementation and controls of the plan as well as the detailed examination of the question of efficient allocation of resources (or of economic rationality) are taken up later, in Chapter 4, once the author has set forth his main thesis. This he does in Chapter 2, where he asserts that the USSR has been forced to pass, sooner that it might have liked to, from the *extensive* to the *intensive* phase of its industrialization. In the first phase accelerated economic development and high rates of growth could be secured via expansion of employment outlets and absorption of existing labor reserves. The underlying demographic data have however, been changed due to enormous war losses, resulting in unfavorable sex and age structure, falling numbers of females and youth of working age, and so on. In the condition of "forced" decreases in labor intakes in industry, and of the current slowing down of the process of transfer of labor from less productive branches (agriculture) to more productive ones (industry), the previous high rates of economic growth could be matched only via sharp increases in productivity, requiring more efficient use of the available means of production, modernization and overhauling of old equipment and installation of new machinery of a labor-saving type (automated plants and automated processes).

This is by now a familiar thesis which has often been discussed and exam-

ined in the West. It should, however, be noted at this point that scheduled improved utilization of the equipment available in agriculture and increased capital inputs in this sector may eventually more than offset the trend of falling rates of growth of industrial labor. Academician Strumilin estimates that by 1965, 12 million surplus farm workers and by 1970, 20 to 30 million surplus workers will become available in agriculture.

On the basis of an assumed labor stringency and the indicated entrance of the Soviet Union into the phase of its intensive industrialization, Boettcher forecasts an increase in real wages. Actually this forecast needs numerous qualifications. Let me note only that the current reforms of the wage system aim both at tightening cost control and increasing work intensity: the work norms are revised while the wage "fund" (*i.e.*, personal plus social wages) is not *increased* but *redistributed*. It is on the other hand true that wide discrepancies occur in practice between planned wage fund and actual payroll.

After examining probable changes in agriculture and industry and stressing the trend toward economic rationality in the intensive phase of industrialization, the author conjectures that in this particular phase of industrialization in the planned economy, just as in the free enterprise economy, coercive controls are bound to disappear. This is, incidentally, contrary to the Hayekian contention that planning is "the road to serfdom" and that coercion and planning are inseparable. Thus both Boettcher and Hayek establish direct though opposite connections between planned economic development and the liberty or freedom of the citizen. Boettcher assumes, somewhat like Isaac Deutscher but starting from other premises, that the whole structure of Soviet institutions must change as the economy penetrates the intensive zone of its industrialization. From stringencies in labor supply to increases in consumer sovereignty, to economic rationality, and to dismantlement of coercive controls, Boettcher passes finally to a so-called dilemma which, he states, faces the Soviet system, namely: How far should this new liberty be expanded in fields other than the economic ones? Alas, it seems to me that Boettcher places much too much weight on the questionable basis of labor shortage and that he goes far afield in his conclusions concerning the liberty of the Soviet citizen. Incidentally, increasing capital costs and the need for larger investments may, in a period of "intensive" industrialization, place new limits on the liberty of the individual as a consumer.

The last part of the work, even more speculative in nature, deals with the connection between Marxian ideology and industrialization, and with the operation of "laws" under socialism. On the question of industrialization, Boettcher oversimplifies the facts. Thus he writes that in the 1920's Trotsky saw the shortest way to socialism in the export of the revolution, Bukharin in the restoration of capitalism (!) (a controlled state capitalism), while Stalin saw it in industrialization. Boettcher lightly passes over the complex controversies on planning and seems to ignore that the Left opposition was designated at the time as "superindustrialist." In respect to the Soviet discussions on the operation of the law of value under socialism the author notes correctly that the Soviet planners and policy-makers have changed their beliefs in the

course of the planning era. The planners have abandoned their early confidence in their unlimited capacity to reshape their economy, and have discovered "objective laws," *i.e.*, the fact that not everything is possible.

NICOLAS SPULBER

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Ekonomicheskiye osnovy narodnokhozyaystvennogo planirovaniya v SSSR. (Economics of National Economic Planning in the USSR.) By A. D. KURSKIY. Moscow: Gospolitizdat, 1959. Pp. 336.

This book consists of five chapters: (1) The law of the systematic development of and the planning of the national economy; (2) Production and construction planning and materials provision; (3) Planned labor and improvements in national welfare; (4) Planning of production expenditures, of prices and financing; and (5) Planned balance in the national economy.

Although the author attempts to present the principles, methods and technicalities of planning the national economy, his coverage is very inadequate. Especially, questions relative to problems of price formation, financing and the budget are treated very superficially.

Quite correct and to the point is the author's remark that "one of the basic principles of Soviet planning is the emphasis on the most important segments of the plan. In the systematic undertaking of this plan a preference is given to the development of those branches of the economy which will assure the fastest expansion of socialist production. To this type of production belong, primarily, all branches of heavy industry. All essential materials, labor and financial resources are concentrated to assure the smooth development of the major segments 'leading links' of the national economy" (p. 55). This statement refers not only to branches of the economy dealing with military requirements, but also to those concerned with scientific research. This, in part, explains Soviet achievements in the field of rocket armament.

Soviet writers are true to themselves when they discuss problems connected, in one way or another, with the Soviet military budget. Though 43 years have passed since the Communist revolution, all questions dealing with Soviet defense remain a state secret. Usually Soviet writers do not say anything at all about the military budget, or what they do say is in sharp contradiction with reality. The author's statement that in 1959 the Soviet government spent 14 per cent of the national budget for defense does not correspond to facts. The truth is that the Soviet unified budget also included the budgets of Union Republics with their local budgets. If local budgets are excluded, that is in relation only to the Union budget, defense expenditures are nearly twice as high. In this connection it is, however, important to note that all principal sections of the Soviet national budget—such as the national economy, sociocultural projects, government and other expenditures—contain larger or smaller sums to be used for defense purposes. Naturally, this information is carefully concealed; but the history of the Soviet budget, and particularly of Soviet budgetary practices, presents ample material to determine approximately how large these sums may be. For example, according to careful estimates, the funds allocated in 1960 for the establishment of defense reserves—

material and food—represent nearly 6 per cent of the gross national product.

Some of the data and assertions of the author are pure propaganda as, for example, his statement that the Soviet Union is using about three-quarters of its gross national product for public consumption (p. 50), or that in 1958 the USSR surpassed the United States in milk and butter production and has thus attained the leading position in the world (p. 145).

Readers of this book will find little that is new, either in terms of data and its interpretation or in terms of approaches and treatment of the problems. What is presented is already well known. There is, however, a certain novelty in some of the franker admissions with which "the socialist economic system" is analyzed—something which earlier would have been impossible.

The author points out that the construction of the Orsk-Khalilovsk metallurgical combine cost 3.1 billion rubles, including incurred losses caused by a protracted building period which amounted to 1.8 billion rubles (p. 188). He also admits that at the present time (1959) the productivity of labor in industry in the USSR is approximately 2 to 2.5 times lower than in the United States, and 3 times lower in agriculture. Actually, the discrepancy in labor productivity is even greater, making it possible for the Soviet Union to constantly claim more significant advances in productivity.

In order to characterize the changes which have taken place in the USSR it may be pointed out that Stalin's name is mentioned in this book only once—whereas Khrushchev's is mentioned thirteen times.

T. Sosnovy

The Library of Congress

Metropolitan Chicago, An Economic Analysis. By Ezra Solomon and Zarko G. Bilbija. Studies in Business, Graduate School of Business, University of Chicago. Glencoe, Ill.: The Free Press, 1959. Pp. xix, 208. $7.50.

This study starts with the proposition that four basic sets of accounts are required for a comprehensive economic analysis of an area: employment, output, income, and saving. Solomon and Bilbija have put together a substantial collection of data, facing the usual difficulties of breaking out regional data, and of constructing sets of regional accounts which tie into national accounts. Without attempting to construct complete sets of accounts, they have presented a generally useful aggregative picture. The text is devoted largely to a descriptive analysis of the Chicago regional economy based upon the above four categories of data. Following are some of the main conclusions:

1. The ratio of the Chicago regional labor force to population is higher than for the country as a whole and for many other urban sections. The high ratio is ascribed to a high proportion of working-age population, and to a high participation of working-age population in the labor force. The reasons for and implications of the high participation rate are not made clear (does it, for instance, indicate a low level of college attendance?).

2. The Chicago regional economy is heavily industrial, with a relatively high proportion of employment in durable goods industries, chiefly metals manufacturing, which contribute 25 per cent of the wage-salary receipts in the region, and nearly 55 per cent of the region's "export receipts." Printing and

publishing, wholesale trade and retail trade are the three other largest contributors to "export income."

3. Productivity per man-hour (in value added) is about 11 per cent above the national average over-all, and 15 per cent above in manufacturing. This is ascribed to several factors, including the relatively advantageous industry-mix and the predominance of industries employing large amounts of capital.

4. Chicago per capita incomes are among the highest in the nation, owing to high labor-force participation, high level of productivity, and relatively high wage rates. Transfer payments are below the national average; this is ascribed to the lower proportion of old people in the Chicago population. Property income per capita approximates the national average and has increased substantially in recent years, but it is still substantially below such older eastern areas as New York, District of Columbia, Massaschusetts, and Connecticut. Labor income per capita is found to be significantly above other leading areas including the New York metropolitian area, Connecticut, New Jersey, and the Philadelphia metropolitian area.

5. Although the data are not conclusive, they show that consumption as a proportion of income is smaller, and savings larger, than for the country as a whole. "In 1955, Chicago had 3.69 per cent of population, 5.00 per cent of disposable income, and generated 7.16 per cent of total personal saving in the nation." The high savings ratio is attributed in part to the fact that a "much smaller proportion of disposable income in Chicago is used in the acquistion of new and used automobiles as compared to the nation as a whole." Personal taxes per capita paid to the federal government are higher than for the nation, but state and local personal taxes are somewhat lower, owing to the smaller use of personal taxes by Illinois and Indiana state and local governments.

Part of the high-level savings has in recent years found an outlet in a higher-than-national-average rate of residential construction. But prior to 1952, the rate of residential construction, relative to population growth, was lower than the national average. Why the shift? The authors find a possible explanation in the behavior of rents; after 1952, rents in Chicago rose much more rapidly than in the rest of the United States (no explanation offered).

6. Indications are that Chicago in the future will depend increasingly on highly specialized production in the metal-working field and in printing and publishing; a relative decline is projected in most other forms of manufacturing activity.

7. The Chicago economy is more recession-sensitive than the country at large, owing to the predominance of durable goods manufacturing and to the fact that secular growth has been somewhat slower than the national average. An interesting sidelight, for which no explanation is advanced, is that in the past two recessions (1954 and 1958) Chicago unemployment reached a peak somewhat later than in the nation.

The study is concerned with the aggregate magnitudes of the whole region and the analysis is heavily on the descriptive side. There is no attempt to break down regional aggregates to show the development of different parts of the region; yet to come are studies with the wealth of insights into the intra-regional structure afforded by the recent studies of the New York metropolitan

region directed by Ray Vernon under the sponsorship of the New York Regional Plan Association.

There is room for argument over methodology at several points. One of this reviewer's principal doubts concerns the use of the old "location quotient" to distinguish between "city-forming" (export) industries and "city-serving" industries. An export balance for an industry X is said to exist if the ratio of Chicago employment in X to Chicago population exceeds the ratio of national employment in X to national population. My opinion is that this dull tool can obscure so many important relationships between the regional economy and the rest of the world, and can lead to so many erroneous impressions, that it is better not used at all. To take a simple case in point, it is conceivable that the analysis might show no export balance, or even an import balance, for a regional industry whose entire output was exported. I hasten to add, however, that the authors are aware of the imperfections of the tool and make some refinements in it designed to remove some of its more obvious crudities.

On the whole, the authors have done well and skillfully what they have set out to do, and their work is a significant contribution to the rapidly growing literature on metropolitan economic analysis.

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Statistical Methods; Econometrics; Social Accounting

Soviet Statistics of Physical Output of Industrial Commodities, Their Compilation and Quality. By GREGORY GROSSMAN. A Study by the National Bureau of Economic Research. Princeton: Princeton University Press, 1960. Pp. xiii, 151. $4.50.

This is the first of a projected series of monographs on economic growth in Soviet industry, agriculture, and transportation as reflected in the statistical record. Logically enough, the first study appraises the statistics themselves. It is limited to individual series of industrial output, excluding the question of aggregation and general indices.

The first part covers the history of the USSR Central Statistical Administration; industrial statistics in the light of Soviet ideology, purposes, and planning; problems of methodological uniformity, definition, and classification; continuous series and small-scale industrial censuses. Two useful charts illustrate the pre- and post-1957 paths by which industrial output data flow from their source in the undertaking to the user agencies and to publication.

The second part appraises the quality of data. It covers Soviet concern with errors and reliability; mechanization (its lack) in data processing; reporting, sources of distortion, and checks to distortion at the enterprise level; distortion at intermediate levels in the economic administrative hierarchy and the statistical system; distortion and ambiguity in published data.

Grossman arrives at the following conclusions: The published statistics of industrial output make some sense and meet certain rough tests of consistency; distortion of information exists at the level of the enterprise but has definite limits; upward distortion is probably more common than downward distor-

tion; distortion is probably much the same from year to year so that time comparisons are not necessarily perilous; unreliable data can often be adjusted to yield more or less satisfactory figures; there is probably no numerical falsification of published data, but suppression, selective release, and ambiguity combine to give a distorted picture of reality.

The author is rather optimistic about making use of Soviet industrial statistics in research, and notes that distortions which receive wide publicity in the USSR and are a definite hazard for the Soviet administrator or planner may be within acceptable limits of tolerance for the outside student.

The exhaustive bibliography highlights the inadequacy of basic source material. It is depressing to note how much information must be culled from Munich émigrés, *Pravda feuilletons*, and even the humor magazine *Krokodil*. Grossman, carefully piecing this jigsaw-puzzle together, has wisely refrained from generalizations which the fragmentary evidence cannot support. As a result, there is no clear conclusion as to the net amount and direction of bias. One might be tempted to twit the author for the apparent contradiction between reporting above-standard production as *brak* (spoilage, rejects) in order to write-down output (p. 82) and counting *brak* as valid finished output in order to write-up production (p. 94); this would be unfair since both things do happen.

As a guide to the American user of Soviet statistics, the volume serves to put the student on guard without being overly discouraging. There is so much concern with methodology, however, that the paucity of actual statistics is underemphasized. The absence of data can be quite as frustrating as the distortions in published material.

The author has felt that comparisions of Soviet and Western methodology were beyond the scope of his study. Sooner or later such comparisons should be made, not to show that one or the other is superior but to show how many problems they have in common. The problem of making an output index for a product which comes in a variety of styles, sizes, and qualities, for example, is by no means exclusively Russian. The author's recognition that this is not a question of communism vs. capitalism but of command economy vs. market economy is very much to the point, as are his brief digressions on command-economy situations in Nazi Germany and in British aircraft production statistics during the second world war.

A somewhat broader survey might have been more appropriate in view of the projected scope of the monographs which it precedes. All the same, Grossman has written a useful and perceptive book.

A. Peter Ruderman

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Econometrics: An Introduction to Maximum Likelihood Methods. By Stefan Valavanis. Edited, from manuscript, by Alfred H. Conrad. New York: McGraw-Hill Book Co., 1959. Pp. xvii, 223. $7.00.

In the past two decades or so we have seen a tremendous development in statistical methods in economics, and it is now extremely difficult for those who are not trained in mathematical statistics to see the real meaning and im-

plications of the recently introduced, highly refined statistical techniques in handling economic models. This is particularly true of the methods for estimating simultaneous linear economic relationships, mainly developed by T. W. Anderson, T. Haavelmo, L. Hurwicz, T. C. Koopmans, J. Marschak, H. Rubin and others who had been more or less associated with the Cowles Commission at the University of Chicago. The book under review was written by a brilliant young econometrician with the intention of introducing modern econometric methods, with an emphasis on simultaneous estimation, to an "intelligent layman" who has no sizable knowledge of mathematics or is not accustomed to thinking in logical terms. The author writes in the Preface that:

> It [the book] has one unifying idea: to reduce to common-sense terms the mathematical statistics on which the theory of econometrics rests. . . . Besides restoring the self-assurance of the ordinary intelligent reader and helping him discriminate between really important developments in econometric method and mere mathematical quibbles, I have tried to be useful to the teachers of econometrics and peripheral subjects by supplying them with material in "pedagogic" form. And lastly, I should like to amuse and surprise the serious or expert econometrician, the connoisseur, by serving him familiar hash in nice new palatable ways but without loss of nutrient substance.

Chapter 1 briefly touches on the fundamental propositions of econometrics and the construction of an econometric model. Chapter 2 goes on to the problem of estimating parameters in the model, and the maximum likelihood method is introduced; some statistical criteria are heuristically discussed. Chapter 3, on bias in models of decay, introduces the concept of *conjugate samples* to discuss the biasedness of the maximum likelihood estimate in an autoregressive single-equation model. The reviewer unfortunately fails to understand the author's claim that the concept of conjugate samples would serve as an example of reducing intricate theorems to common-sense terms.

In six chapters, 4 to 9, the author discusses the estimation of simultaneous linear structures—a central theme of modern econometric methods; the exposition, in particular with reference to the concept of identification, is extremely lucid. The treatment of limited-information maximum likelihood estimation in Chapter 8, however, is rather ambiguous. The limited-information method is explained in terms of a water-pipe problem. It seems to the reviewer that the author's illustration is equally applicable to many similar problems; it does not necessarily illustrate the reason why the limited-information estimate has to take the form derived by T. W. Anderson and H. Rubin.

The last three chapters are devoted to various topics in econometrics, such as linear confluence, partial correlation, bunch-map analysis, factor analysis, correlograms, seasonal variations, trends, and diffusion indices. The subjects are selected *ad hoc* and the exposition is superficial. Appendices cover technical aspects of some of the subjects discussed in the main text.

Perhaps because of the author's death before the book went through the press, there are some minor technical mistakes and ambiguities (*e.g.*, in the treatment of underlying assumptions or of identification), but the book as a

whole is well organized and the references for further reading at the end of each chapter are carefully selected; it is not only a pleasure to read, but the reader may be able to get a fairly well-balanced glimpse of what modern econometric methods are about. The author has realized surprisingly well his intentions as stated in the Preface, except for the last one. To those who are seriously interested in the subject, however, the book is hardly recommended. The common-sense interpretation, as presented in this book, is unreliable and superficial; it easily leads to many fallacies. After all, the mathematics required to understand the now classical Hood and Koopmans' *Studies in Econometric Methods* (New York, 1953), reviewed by M. Hastay in the *American Economic Review*, March 1954, are not terribly difficult to master; and the reviewer believes that the principle of round-about production may well be applicable in this case.

HIROFUMI UZAWA

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Preface to Econometrics. By MICHAEL J. BRENNAN, JR. Cincinnati: South-Western Publishing Co., 1960. Pp. x, 419. $6.50.

Econometrics, which has gradually emerged from its esoteric status to become a widely used method of economic research, has unfortunately not yet succeeded in gaining acceptance as a standard subject in the undergraduate curriculum. This fact means not only a significant gap in economic education, but also frequent frustration on the part of students, who are now encountering more and more econometric terminology in books and periodicals. A fundamental condition for the widespread offering of econometrics at the undergraduate level is of course the cultivation of a sufficient number of qualified teachers, but the publication of suitable textbooks in this field is a prerequisite no less important. For this reason alone, the book under review should be a welcome contribution.

Aimed at readers without advanced mathematical background, this text presupposes only a knowledge of elementary algebra, elementary statistics and basic economic theory. The main body of the book is, besides, preceded by a well-written introduction in which the author (a) clarifies the differences between econometrics on the one hand, and mathematical economics and statistical economics on the other; and (b) outlines the general procedure of scientific investigation and model construction. This prepares the readers for the chief contents of the book which are divided into five parts.

Part I is concerned with variables, functions, systems of equations and determinants, and Part II presents the basic elements of calculus. Together, the first two parts occupy half the length of the book. After a short general discussion of econometric models in Part III, methods of statistical inference are then explained in Part IV, where the author devotes about a hundred pages to probability theory, sample theory, regression and correlation analysis. The problem of autocorrelation and the mean-square-successive-difference method of testing its existence are also treated in this part in connection with time series. Finally, in Part V, some recent developments and specific aspects of

econometrics are discussed, including the problem of identification, and decision under uncertainty. There is a four-page bibliography at the end.

From a pedagogical viewpoint, this book has considerable merit. Throughout the volume, the discussion of each set of mathematical techniques is followed immediately by illustrations of practical applications. This enables the students to sustain their interest by constantly observing the relevance of mathematical methods to economics. Exercises—of which this reviewer wishes there were more—are given in most chapters, and answers to odd-numbered problems are supplied by the author. The students can thus check the correctness of their own solutions without having to consult the instructor. As to exposition, except for certain places where the author shows a tendency to achieve brevity at the expense of clarity, the writing is very lucid.

The distribution of space among the various topics, however, leaves something to be desired. Recognizing that the formulation of theoretical models is an essential step in econometrics, this reviewer nonetheless feels that the weight (50 per cent) assigned to mathematical economics as against statistical inference is too heavy. This belief is strengthened by the fact that some very elementary materials in the first half of the book are given unnecessarily detailed explanations (*e.g.*, functions, graphs and simultaneous equations), whereas some more difficult materials in the second half are dealt with either too briefly to be intelligible (*e.g.*, the rank criterion of identification) or too superficially to be of real use to the students (*e.g.*, multicollinearity). Many aspects of statistical theory, it would seem, could well stand some amplification; if accompanied by an appropriate reduction of the first two parts, this would substantially enhance the usefulness of the book without unduly adding to its size.

Another flaw lies in the author's failure to mention the names of certain mathematical techniques discussed in the text, such as Cramer's Rule (p. 98), Lagrange multiplier (p. 183), and the rank criterion of identification (p. 380). It is difficult to understand why the author would want to deny the readers the knowledge of convenient, nay, standard ways of referring to these methods.

In general, however, Brennan's book will serve its intended purposes well. Indeed, the relative scarcity of suitable textbooks in this field adaptable to undergraduate instruction should make it a most promising candidate for adoption in courses in mathematical economics (using the first half of the book) as well as econometrics (using the entire book, with emphasis on the second half).

ALPHA C. CHIANG

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Economic Systems; Planning and Reform; Cooperation

Ekonomicheskaya effektivnost kapital'nykh vlozhenii i novoi tekhniki. (The Economic Effectiveness of Capital Investment and New Technology.) Edited by T. S. KHACHATUROV. Moscow: Izdatel'stvo Sotsial'no-Ekonomicheskoi Literatury, 1959. Pp. 615. Rbl. 16.20.

This book represents one more episode in the struggle between the mon-

goose of logic and the cobra of ideology that has raged among Soviet economic theorists for the past three decades. Specifically, it is another episode in the debate concerning criteria for rational investment choice. The essence of this debate, as Grossman has characterized it, is the "conflict . . . between the Marxian conception of value and the logic of the allocation of scarce resources to achieve posited goals; more precisely between the absence of a value attached to capital *as such*, and the necessity for Soviet planners to husband their very scarce capital resources."[1]

The conflict began when the interest rate was purged as being un-Marxist in the early 'thirties. But no capital charge at all is equivalent to an effective interest rate of zero. It requires no especial genius to perceive that a zero interest rate leads to absurdly capital-intensive investment decisions in the context of a capital-poor country. To avoid this, project-making engineers employed various surrogates for the interest rate on an *ad hoc* basis throughout the 'thirties and 'forties. After the war, the use of such quasi-interest rates (the most common was euphemistically dubbed the "coefficient of relative effectiveness," here abbreviated as CRE) and the economists who sought to justify those rates, fell under savage attack from those guardians of doctrinal purity, the political economists. Charges of everything from logical inconsistency to political deviation were hurled at the defenders of the CRE. "Since the end of 1949," wrote Grossman in 1953, "the controversy has remained smoldering, flaring up occasionally in isolated sectors, producing recantations and reaffirmations, rejoiners and retreats. . . . the air is still full of uncertainty, with no discernible signs of resolution either by intellectual agreement or by authoritative dispensation."[2]

Certain engineers surreptitiously continued to use the discredited CRE or its reciprocal, the period of recoupment (POR), but they were handicapped by the absence of any officially prescribed minimum rate or maximum pay-off period. The result was that the POR used in practice varied from 3 to 25 years. Other project makers made a practice of choosing the alternative that minimized current operating costs. There was, however, a general cognizance that scarce capital was frequently being misallocated. After Stalin's death in 1953, several unsucessful attempts were made to resolve the issue, but, by 1958, the situation had not significantly improved.

In an attempt to improve the unsatisfactory state of affairs, there was convened in Moscow in June 1958, the All-Union Scientific-Technical Conference on Problems of Determining the Economic Effectiveness of Capital Investment in New Technology in the Economy of the USSR.[3] The book here reviewed contains 40 papers presented at this conference, 37 shorter contributions to the discussion, and a list of the conference's recommendations.

The chief immediately positive results of the conference were its recom-

[1] Gregory Grossman, "Scarce Capital and Soviet Doctrine," *Quart. Jour. Econ.*, Aug. 1953, *67*, 311-12.

[2] *Ibid.*, p. 337.

[3] The papers were summarized in *Voprosy Ekonomiki*, 1958, No. 9, pp. 119-62.

mendations; these laid the basis for the recently published "Standard Method of Determining the Economic Effectiveness of Capital Investment and New Technology in the National Economy of the USSR."[4] Substantially, the recommendations embrace the position of T. S. Khachaturov, a veteran of the controversy who headed the organizational committee of conference and edited this book.

The recommendations finally accorded the rights of Soviet citizenship to the coefficient of effectiveness. Since the CRE has been in standard, albeit *sub rosa,* use for a quarter century, this final blessing can hardly be termed an undiluted creative triumph for Soviet political economy. Of greater practical significance was the specification of how the normative coefficient of effectiveness (NCE) should be determined. According to the recommendations, the coefficient of effectiveness is to be calculated on three levels: (1) The coefficient of absolute effectiveness (CAE) is the ratio of the annual physical growth of national income to the capital that brought about that growth. This definition is ambiguous in that it is not clear whether the CAE is equal to the ratio of *all* the increase in output in a given year to the increase (lagged) in capital stock (the marginal output-capital ratio), or to the partial derivative of output with respect to capital (marginal productivity of capital). Additional ambiguity is introduced by the confusion over the proper weights to be used in aggregating the diverse physical components of "national income." (2) The NCE for each branch of the economy is to be established in such a way as to equilibrate the exogenously determined volume of investible funds available to each branch with the demand for those funds within the branch. (3) The CRE comeasures the initial capital outlays and the average annual operating outlays for different technological variants of producing a *given stream of output*. As formulated, the CRE is applicable not to decisions about *what* to produce, but only to decisions about *how* to produce a posited stream of output.

Another step forward was the rehabilitation of compound interest, long anathema, as a means of rendering commensurable capital outlays that may occur at different points of time. Its use is limited to this application since operating costs are taken as an annual average, and output is identical for all technologies.

Essentially, the formal conclusions of the conference, as embodied in the recommendations, have restored matters to what they were before the onslaught of the doctrinal purists. Aside from legitimizing the CRE, the most positive contribution of the conference was to establish rules for computing the NCE for each branch, and to clarify the manner of calculating the CRE. Much, of course, remains to be done. The extent to which the coefficient of effectiveness should enter into decisions as to the composition of the final bill of goods remains unspecified. The degree to which indirect capital requirements of alternative technologies should be taken into account is vague. The concepts of the productivity of investment, and the opportunity cost of its use

[4] *Tipovaya metodika opredeleniya ekonomicheskoy effektivnosti kapital'nykh vlozhenyi i novoi tekhniki v narodnom khozyaistve SSSR*. Moscow: Gosplanizdat, 1960. Pp. 21.

are confused. Logical inconsistencies pervade the method of calculating the CRE; *e.g.*, the practice of including the undepreciated part of an existing machine's initial cost in the capital outlay of the new machine whose installation as a replacement is being considered. Considerations of obsolescence and replacement are hopelessly confused, and the factors of uncertainty and time horizons are entirely ignored. The most serious shortcoming of all is that the irrational system of Soviet price formation remains unchallenged in the conference's recommendations. Recognition of this irrationality is implicit in the admonition that all conclusions reached on the basis of CRE calculations must be qualified by qualitative considerations of "deficitness" of certain inputs.

It is impossible here to do justice to the various papers presented at the conference. Most significant was the atmosphere of the debate; charges of political deviation and ideological treachery were entirely absent. The extreme critics of the CRE, such as P. S. Mstislavskii, were not there; the moderates, typical of whom is Khachaturov, carried the day. And the radicals were well represented by Vaag, Atlas, Malyshev, Lur'e, Novozhilov, and Kantorovich. It may be significant that Academician V. S. Nemchinov, certainly one of the most powerful and influential Soviet economists, lent his support to Novozhilov's ingenious scheme to rationalize the whole Soviet price structure.

It is a pleasure to report that the mongoose appears finally to be getting the better of the cobra.

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Value and Plan: Economic Calculation and Organization in Eastern Europe. Edited by GREGORY GROSSMAN. Berkeley and Los Angeles: University of California Press, 1960. Pp. viii, 370. $7.00.

The years since the death of Stalin have found the Western world paying increased attention to what is going on in the Soviet Union and Eastern Europe, particularly in the field of economics. The volume here reviewed, a study of the nature of the Soviet-type economic system, is a case in point.

More than anything else, this volume is a response to the fact that Soviet (as well as Polish and Yugoslav) economists in the post-Stalin period have been writing in greater volume and with more candor about their system than at any time since the early 1930's. These primary source materials of a theoretical nature warrant analysis and evaluation in their own right; but they also supplement the descriptive and statistical information on which we continue to rely heavily for interpreting Soviet economic performance and the nature of the system. All of these approaches to an understanding of basic Soviet economics appear in this excellent collection of 14 essays which were originally presented at a 1958 conference of 50 economists and one sociologist held in Berkeley.

With the exception of the essay by the sociologist Reinhard Bendix on the cultural and political setting of economic rationality, the volume is an examination of economic systems and problems from a strictly economist point of

view. For the most part, this view is the relatively static one of price, production and distribution theories, with the result that the question receiving most attention is whether economic decision-making in the Soviet Union and Eastern Europe satisfies "optimal" conditions and is therefore "rational." This focus is laudable (however limited) because the theoretical issues are complex—the more so in a comparative study. They therefore fully merit the careful and discursive treatment they receive. One effect of this orientation is that the volume is a good deal more cohesive than otherwise might be expected from 14 essays of individual authors—a tribute also to the skill of Gregory Grossman, who conducted the conference and saw the essays into print.

By the same token, however, the volume is forced to deal only secondarily with the dynamic problems of industrialization and economic development. But even here, the clarification of basic theoretical issues (in static terms), together with the exploration of problems of economic structure and organization, throws considerable light on the process of economic change under Soviet conditions.

Is there an underlying rationality to economic decision-making in the Soviet Union and Eastern Europe? Most of the authors think so, but differ as to the meaning of the expression and the assumptions that ought to be made. Concerning the nature and expression of "final preferences," for example—Alfred Zauberman sees a continuing dominance of the planners over the consumers; P. J. D. Wiles doubts that the planners know what they want; J. S. Berliner suggests that they need to have only a rough idea of what they want; and Leonid Hurwicz and others simply build a "planners' utility function" into their discussions.

Concerning the problem of resource allocation—all participants see a growing awareness on the part of Soviet (and, to a greater extent, Polish and Yugoslav) economists of the need for criteria which accurately reflect real scarcity relationships. The basic ideological dilemma is discussed by Zauberman, centering on the role of the "law of value" under socialism; J. M. Montias traces in fascinating detail the development of ideas on the pricing of material inputs and investment goods in Poland; D. Granick, J. P. Hardt and Holland Hunter respectively discuss related problems for the Soviet Union in industrial technology, electric power and transportation, and location; and R. W. Campbell evaluates Soviet accounting methods. If there is a consensus in the matter, it is that broad factor interrelationships have somehow made their influence felt, despite the crudity of decision-making instruments, and that there consequently might be a danger in overestimating the practical effects of the theoretical modifications currently being proposed. (Considering the interest in problems of resource allocation in this volume, it is unfortunate that no real attention is given to the related questions of wage determination and labor recruitment.)

The recognition that rationality of economic decision-making involves the totality of interrelationships between final preferences and resources, leads much of the discussion to the question of economic structure, centering on the implications of the 1957 reorganization of the Soviet administrative and planning framework. Michael Kaser describes the essential features of the reor-

ganization, and R. Bićanić presents comparable information on the structure of the Yugoslav economy.

The theoretical and practical problems of economic organization are formulated in a variety of ways—in terms of centralization vs. decentralization, "command" vs. "market" control, the production principle vs. the territorial principle, the function of prices, and the role of the enterprise. Wiles, Hurwicz and Benjamin Ward discuss various models; Donald Hodgman examines the particular problem of monetary controls under central planning; and everyone else makes a contribution from his own particular vantage point.

The general conclusion is that if economic decision-making is to become more "rational" in the Soviet Union, it will be through refinements and modifications within the essentially centralized structure, rather than through the use of prices or other indirect guides to microeconomic behavior, such as in Yugoslavia. The vital question of whether the Soviets actually will be able to bring about these refinements and modifications, so as to increase the effectiveness of economic planning and administration, remains essentially unanswered, although the contributors to this volume see tangible progress in this direction.

WARREN W. EASON

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Business Fluctuations

The Wage-Price Problem. *By* JOHN M. CLARK. [New York]: The American Bankers Association, 1960. Pp. vi, 68.

This short monograph by the dean of American economists was commissioned by the Committee for Economic Growth without Inflation of the American Bankers Association to provide an "objective, balanced synthesis" of recent thinking on the wage-price spiral. The monograph covers familiar territory and says very little that has not already been said many times. What distinguishes it from many of the others on the same subject is that it is calm, reasonable and, of course, beautifully written.

Professor Clark joins the long list of economists who believe that there is such a thing as a "pushed-up" inflation, that labor and management are jointly responsible for it, that it has been in evidence in recent years in the United States, and that steps should be taken to control it. Although it is much more moderate than the classical demand-pull inflation, the new inflation is cause for concern because it may be a problem for an indefinitely long period. However, he does *not* say that the new inflation will necessarily be converted into a gallop or that it will significantly impair growth. He objects to it largely on the ground that it will have undesirable distributional effects and may impair the nation's competitive position in international markets. A spontaneously stable price level would be more favorable to growth than creeping inflation, but he prefers a mild inflation to the consequences of the restrictions that would be necessary to stamp it out.

Since the pace of the new inflation in this country has been slow, Clark sees no reason for trying to eliminate it overnight, but he believes it would be

desirable to take more effective action than has been taken thus far. Restrictive monetary and fiscal policies should not be used because they create excessive unemployment. Wartime price and wage controls might be fatal to the essential character of the free enterprise system; in any case, they would probably not be able to cope with a slow upward price creep. Fragmentation of oligopolistic industries is probably impractical; moreover, there is considerable doubt that it would accomplish very much. Breaking up labor unions would be "unthinkable" on political grounds, while other feasible action in the labor field, e.g., internal reform of union abuses, might increase rather than decrease the cost-raising pressures of unions. In the end, Clark is left with voluntary restraints and persuasion to deal with this intractable problem. He urges experimentation on a wide front, using a combination of investigation, consultation and education by government fact-finding boards, cabinet committees, and private research organizations to induce voluntary adoption of less inflationary behavior on the part of unions and management. If this is a mild and somewhat anticlimactic recommendation, Clark reminds us that "remedies should be proportionate to the severity of the problem."

Joseph A. Pechman

The Brookings Institution

Der Konjunkturtest: Eine neue Methode der Wirtschaftsbeobachtung. By Wilhelm Marquardt and Werner Strigel. Schriftenreihe des Ifo-Institutes für Wirtschaftsforschung no. 38. Berlin: Duncker & Humblot, 1959. Pp. 223. DM 28.—.

Statistics, in spite of all efforts to achieve greater speed and broader coverage, has not succeeded so far in meeting the continuously growing demands of cyclical and market research. Since the reason for this does not lie in a lack of intelligence or diligence of statisticians but is mainly due to the limitations of the instruments for collecting data, it was necessary to find a simplified procedure for supplementing traditional statistics by sending out an advance patrol—a patrol which might yield deeper insight than the rarely reliable advance-guard of "preliminary" figures.

In order to fill this gap the Ifo-Institute for Economic Research, Munich, started ten years ago in West Germany a novel monthly questionnaire called *Konjunkturtest* (business test). This especially arranged survey serves to obtain economic facts beyond those which up to now had been collected by official statistics and professional organizations. The method is to gather statements at monthly intervals from a representative number of firms about the tendencies of selected economic variables in various industries and trades and about the views and expectations of entrepreneurs. Quantitative statements are not asked (*"statistique sans chiffres"*). The simplified questions aim to find tendencies only, mainly changes as against the preceding month, *i.e.*, increase, decrease, or no change.

Up to 1958 this method of obtaining a system of short-term economic indicators had been adopted by 21 countries, 12 of which apply it in the orginal Munich way, while the others, mostly following a French pattern, use slightly divergent procedures. The amazing outcome—besides the rapid dissemination

of the results of a rather simple survey technique—is the unexpected fertility of the business-test approach that has been revealed for so many fields of study in statistics, economics and econometrics. The quantification of qualitative information, the aggregation of microeconomic data, the sample design are all problems of statistical importance which are dealt with adequately in this book. Because the results for different variables supplement and control each other and can be checked, additionally, by certain other data, they have proved suitable for giving an up-to-date and valid picture of reality.

The usefulness of the method for economic diagnosis has been studied and recognized by such an important body as the Economic Commission for Europe, Geneva. It is true, however, that, so far, in a period characterized by steady upward trends it could not be proved conclusively that the test approach is more appropriate than other methods for finding the turning points of the cycle. But that is no serious objection against making an effort to broaden and improve our empirical knowledge as best we can.

Beyond its practical use the business test facilitates model building and the testing of economic hypotheses by providing empirical data which otherwise would hardly be available. It deals with *ex ante* and *ex post* relations on a microeconomic as well as on a macroeconomic level. By learning about the reaction patterns of entrepreneurs the investigator also learns what is the forecasting reliability of their expectations and anticipations and is protected from taking them at their face value.

The authors of the present book are very competent to treat this subject because they have been in charge of the current survey in the Munich Institute since its beginning. They give a comprehensive description of the motives, the techniques, and the history of the development of the business-test method in West Germany and abroad. They critically evaluate the results hitherto obtained, but at the same time they are very optimistic with regard to the prospects of further research. They offer an outlook on those theoretical issues on which the business test seems to throw light. Their lucid presentation is backed by numerous tables, figures, and references to a vast literature. The book is well furnished with a bibliography of 100 monographs (58 in German, 26 in English, 9 in French, 7 in other languages).

LOTHAR BOSSE

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

British Monetary Experiments 1650-1710. By J. KEITH HORSEFIELD. Cambridge: Harvard University Press, 1960. Pp. xix, 344. $7.50.

Textbooks frequently begin the economic history of modern Britain with the industrial revolution and the "new economics" of Adam Smith. By contrast, the pre-industrial era of the sixteenth and seventeenth centuries has been referred to by F. J. Fisher as "The Dark Ages in English economic his-

tory."[1] Historical continuity has been weakened by this division, not so much by changes in historical interpretation as by changes in historical method. In brief, a widening chasm has been observed between the literary-humanist approach to the earlier period and that of quantitative measurement and economic analysis to the latter period.

Mr. Horsefield has combined these two methods with remarkable success in his study of British monetary experiments. On the side of literary source materials, a new pamphlet literature grew up after the Restoration which reflected the vigor and innovating propensity of the age. The vast quantity of this material—over 600 items in the author's bibliography—reminds us that Dr. Johnson's expletive, "The age is running mad after innovation. . . ." is equally descriptive of Defoe's England. Among the contributors to this monetary literature were Newton, Locke, Defoe, Wren, Petty, North, Cary, Law, Barbon and Child. Statistical materials—both public and private—also became more abundant after the Restoration. In Part I the author provides a statistical framework for his study in terms of price and monetary data. This, together with the tools of economic analysis, enables him to evaluate the merits of numerous monetary proposals.

An introductory chapter reviews the chief monetary developments of the period 1650-1710. Most attention is devoted to these developments in the decade of the 1690's, and especially the years 1694-96. These three climactic years witnessed the founding of the Bank of England and the Bank of Scotland, the recoinage of silver, the revaluation of gold coins, the establishment of land banks, the suspension of cash payments by the Bank of England, and the introduction of Exchequer Bills.

Part II is concerned with silver and the controversy over recoinage which reached a height in 1695. The "Locke-Lowndes" controversy over possible devaluation of the currency is considered chiefly from the standpoint of inflation, deflation, and the balance of payments. In the end the Locke school won, and the recoinage of 1696 restored silver to its previous mint standard, not, however, without economic hardships. Besides silver, the gold guinea (Part III) was a supplementary currency whose behavior in the 1690's is reminiscent of the bimetallic problem of the nineteenth century.

Part IV takes up a variety of paper money schemes. The projectors of these schemes were unanimous in their claims that banks would increase the supply of purchasing power, provide working capital for an underemployed labor force, and thus stimulate manufacturing and trade. Horsefield says that most of these projects "were based on tangible assets such as commodities or land, or with predictable money flows such as the public revenue. Not until the last quinquennium of the century was it seriously contemplated that a public bank might have a proportionate reserve, as did the goldsmiths."

Of the seven types of banks that were proposed from 1650 onwards, the author singles out five for special treatment: First, Lumbard (or Lombard) banks, an outgrowth of pawnbroking, issued notes on the security of goods.

[1] F. J. Fisher, "The Sixteenth and Seventeenth Centuries: The Dark Ages in English Economic History?" *Economica*, New Series, Vol. XXIV, No. 93 (February, 1957), pp. 2-18.

Second, there were sixty-odd plans for capitalizing government revenues. Third, the Bank of England is viewed within the context of numerous monetary experiments and the contemporary economic scene. The reader will gain new insights into the evolution of the project which became the Bank, the expansion of its note-issue, the nature of its assets, and the extent to which it functioned as a central bank. Fourth, there were other projects for commercial banks, such as country banks and schemes for transferring funds from place to place. The Orphans' Bank, Million Bank, and Bank of Scotland are included in this category. The fifth type consisted of land banks.

Horsefield is interested in the monetary experiments that failed as well as those that succeeded. This is not merely an antiquarian interest, for he finds that nearly all of the innovations emerged from the writings of men whose projects met with failure. This applies to four land banks, of which three were based on the ideas of Hugh Chamberlen, who planned to issue legal-tender notes representing the capitalized value of future rents. Despite specious arithmetic and the lack of provision for cash reserves, Chamberlen's projects enlisted considerable support from English landlords.

The author's summary and conclusions appear in Part V. With reference to money, gold and international trade, he finds writers who among other things went a long way towards understanding the quantity theory of money and the relationship between purchasing power parity and the exchange rate. With reference to banking, paper money and inflation, answers from contemporary writings are found to such questions as what is a bank for, what were the essentials of a bank, why did the Bank of England and the Bank of Scotland succeed, why did no such institutions emerge before 1694, and why did none successfully follow them. Ten appendices and a most exhaustive bibliography complete this work.

By combining the methods of historical investigation and economic analysis, Horsefield has made an excellent contribution to monetary history and the history of economic thought.

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Politica monetaria. By GIUSEPPE MIRABELLA. Palermo: Seminario di Economia Politica e Scienza delle Finanze dell'Università degli Studi di Palermo, 1959. Pp. 330.

The primary objective of this study is to analyze the changes which have occurred in the development of monetary policy during the past decades—from an earlier, almost total reliance on discount and interest rates as instruments of monetary and credit policy to the growing variety of regulatory weapons currently at the disposal of monetary managers.

Recognizing the importance of financial factors in the shaping of current economic events, Professor Mirabella begins his analysis with a review of the efforts made in recent years by economic and monetary theorists to understand more fully and to define in more precise quantitative terms the factors involved in the achievement and maintenance of monetary equilibrium. In that

connection he presents, in the first part of his monograph, a detailed and thoughtful summary of the work of Koopmans, Holtrop, Witteveen, Tinbergen and of recent OEEC studies. This is followed, in the second part, by a discerning discussion of the nature and functions of the principal instruments of monetary and credit policy—from orthodox discount-rate changes and gold flows of gold standard days to changes in reserve requirements, open-market operations, the setting of liquidity ratios, the imposition of direct credit controls and the other means of monetary management currently at the disposal of central banks.

The third and final part discusses the general institutional environment in which central bank policies and monetary controls are currently applied in practice. Attention is given to such factors as the expanding role of fiscal policy in the economic life of the community and the problems which its growing importance poses for monetary managers, and the lessened responsiveness of demand and supply of credit to changes in interest rates because of the emergence in the economy of new rigidities and new institutions and practices—such as finance companies supported by powerful industrial groups independently able to provide cheap consumer credit, long-term financing provided through revolving short-term credits, the growing volume of corporate savings available for investment in the enterprises within which they originate, the effects of pressures by powerful labor groups upon certain basic costs and prices, and finally current tendencies toward direct or indirect interventions and pressures by powerful political groups in the regulatory activities of monetary authorities.

Although generally no specific solutions of his own are offered by the author for the problems and difficulties which he describes, his comprehensive and scholarly review of the problems currently encountered in the field of monetary management and policy provides suggestive and stimulating reading for the student of monetary affairs. The usefulness of the monograph is enhanced by apt and frequent footnote references to the work of others and by the comprehensive bibliography appended to the volume.

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Money, Banking, and Economic Welfare. By Paul B. Trescott. New York: McGraw-Hill Book Co., 1960. Pp. xii, 578. $7.50.

Designed for the undergraduate course in money and banking, this new text covers much the same ground as Chandler's *The Economics of Money and Banking*. The conventional material on the nature and evolution of money and on the mechanics of monetary transactions is presented in Parts I and II. A major section on monetary theory (Part III) is followed by two chapters (Part IV) on financial institutions and their role in the saving-investment process, and two chapters (Part V) on international relationships. The author's interest in history manifests itself in a major section (Part VI) recounting money and banking developments from colonial to present times. The final part is devoted to a discussion of current monetary problems and an

appraisal of means for coping with them. In general the author's style is orderly and clear.

Several specific features set this book apart from others in the field. Considerable emphasis is given to the theoretical distinction between the "main money flow" (payments for current production and services) and the "financial circulation." The relationships between the two are developed through the use of a budget equation to which each transactor must conform: income plus borrowing equals expenditure plus increased cash holdings plus increased holdings of other financial assets. Only a page (p. 115) is given, however, to a description of the institutional arrangements of the money market and the types of instruments and transactions employed by economic units wishing to adjust their liquidity positions.

The text's treatment of general equilibrium in Chapter 9 is distinctive. This synthesis is presented in the form of algebraic equations which recognize time-lags, and this enables Trescott to analyze disturbances to equilibrium as a dynamic process. Those students who are willing to make the effort required to trace through the process of period-by-period adjustment will receive, I believe, a rich reward in the form of a comprehensive understanding of the mutual dependencies which constitute an economic system. The author is to be commended, too, for a willingness to confront theoretical formulations with empirical evidence. Although users of the book may dispute some aspects of the text's interpretation of the empirical record (*e.g.*, the data relating borrowing and interest rates on page 202) this material will nevertheless be useful for classroom discussion.

The chapters on monetary history I found less valuable than other sections of the book. Here the author has laid out an intricate, detailed, chronology of currency issues, types of coinage, detailed aspects of silver legislation, and so on, covering a span of 300 years, and the sheer volume of detail has forced the author into a rather superficial treatment. Although the theoretical tools of earlier chapters are at the disposal of the reader, very little use is made of them in interpreting the historical record.

What I regard as a deficiency in both the theory and policy chapters is the inadequate treatment of the role of uncertainty. There is no recognition, for example, that a policy of permitting or inducing short-term fluctuations in the prices of government securities which are not amenable to prediction by the public, as contrasted with a policy of price stabilization, would reduce the "moneyness" of the public's holdings of these securities and hence, other factors constant, increase the demand for cash balances. Trescott's view of the significance of monetary policy is limited to its effects on factors operating on the supply side of money markets, i.e., on the quantity of money, its cost, and the extent of credit rationing by lenders (p. 534).

Because much of Trescott's text is appealing, I regret having to report the presence of a fairly large number of inaccuracies. On page 188, the total demand for money is denoted by M; later (p. 204) the symbol is switched to D_m. But the latter symbol has already been used on page 186 as meaning the transaction demand only. On page 157, "propensity to consume" would more

helpfully be stated as the marginal propensity. Here, too, the author states that "multiplier analysis implies that investment changes infrequently and stays constant for long periods despite large changes in consumption" whereas it would be fairer to say that multiplier analysis is only a partial explanation. The example showing how the marginal efficiency of capital should be calculated (p. 160) is not accurate; use of the present-value formula presented on an earlier page (p. 117) would have straightened this out.

The definition of liquid assets (p. 186) makes them out to be assets whose "money value does not fluctuate" (among other attributes) which in general is true only of money itself. And on the following page reference is made to choosing "between liquid assets and such other stores of value as . . . bonds. . . ." On page 188, bonds are put back into the category of "liquid assets." One reads on page 104 that government transfer payments "are income payments . . . for which the government receives no currently produced goods or services in exchange," a statement calculated to worry the reader about the concept of income. The drop in GNP during the 1953-54 and 1957-58 recessions I figure to be 2.7 and 3.6 per cent respectively (using quarterly data) not the "less than 1%" decline described on page 128.

I question, too, the author's emphasis that "business disposable income" (depreciation plus undistributed profits) "has generally been stable at about 10% of GNP" (p. 146) and "In the 1950's BDI consistently accounted for about 10% of GNP" (p. 110); this is likely to lead to a misapprehension by students concerning the stability of corporate profits. My calculations show that during the period 1951 to 1958 the range of maximum departure of business disposable income from the 10 per cent of GNP level, using quarterly data, was over $6 billions.

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Public Finance; Fiscal Policy

Public Finance and Full Employment with Special Reference to Underdeveloped Areas. By V. V. BORKAR. University of Bombay Publications, Economic Series, No. 9. Bombay: Vora and Co., 1959. Pp. xlv, 179. Rs 12.50.

Fiscal Policy in Underdeveloped Countries, with Special Reference to India. By RAJA J. CHELLIAH. New York: Macmillan, 1960. Pp. 168. $3.50.

In these books two economists with evidently strong humanitarian instincts use the tools of modern economic analysis to study a problem important to mankind. (Neither, however, gives any explicit attention to the query, "What do the people want?" except a brief remark by Borkar that some Indians want freedom to be lazy [p. 140].) Although the volumes have much in common, they also differ substantially. Borkar's is the more "theoretical" and makes the broader claims to generality of application. Chelliah's focuses more closely on concrete problems, while drawing on a solid foundation of theory.

Dr. Borkar, of Kanatak University, sets himself the task of extending or

adapting Keynesian analysis to the economies of underdeveloped lands. His basic argument was completed several years ago and seems less original today than it probably was when written, *e.g.*, the emphasis upon the need to coordinate monetary and fiscal policies. The references and citations suggest that heavy reliance was placed on materials we should no longer consider most advanced. The interpretations of developed economies leave something to be desired; it would be a pity if Indian students accepted as true of the United States some of the "asides" in this book, statements which are not essential to the author's argument.

Borkar defines *full employment* as "a situation in which the demand for labor matches with the supply of labor of those willing to work at a wage rate equal to their marginal productivity, not less than the cost of living. . . . *Cost of living* denotes the minimum income which it is considered desirable to ensure to all. . . . Obviously this is a flexible concept. . . . Where exactly we draw this line does not, however, affect the analysis materially." (It might affect expectations and policy, I should think.) The definition is designed to force attention to disguised unemployment in which the wage rate equal to marginal productivity is less than the cost-of-living level. We may question the use of definitions no more precise than these. Yet my own doubts, I admit, are of minor significance alongside the importance of the major theme—that investment must be stimulated, and the "right" kind of investment. The concerns which Keynes had about investment differ profoundly from those facing India.

Government has a major role to play in investment for six reasons (pp. 48-51)—largely to fill big gaps left by private investment. Fiscal and monetary policy must be directed toward stimulating total investment. For reasons developed at length (Ch. 5), Borkar concludes that in India monetary policy can be expected to accomplish little in speeding investment. Deficit spending must be used, but with caution. The discussion of the economic effects of deficit spending and of the growth of public debt in India can probably be applied to other underdeveloped lands. Except for perhaps inadequate attention to balance-of-payments problems and, to my mind, undue worry about the effects of growing debt on the distribution of wealth (what would the bond buyers have done otherwise?) this discussion is probably a real contribution. Inflation is an evil which, in Borkar's view, India must not tolerate. Heavier use of indirect taxes is recommended—and, by implication, a tax to be paid in labor.

A final chapter on fiscal policy in India since 1938 abounds with criticism. Throughout the first 142 pages of the book I often asked myself, in effect, "how can the men running the government of a poor country have the competence and integrity to do the job Borkar expects of them?" On the basis of his interpretation of the record, they have not done well, although recent policies are better than those before about 1956. I remain with the feeling that the author's fiscal-policy programs with their heavy emphasis on government investment require a quality of government—in Parliament and in the Civil Service—not likely to be found soon in underdeveloped countries. According

to the author, Indians acting through the market process do not do enough to speed growth. I wonder how much better they can be expected to do through the political process. Fortunately, they have in Borkar's study much that can help them.

Dr. Chelliah of the National Council of Applied Economic Research in New Delhi also concentrates on the need for greater capital formation. He, too, is impressed by the difficulty of applying Keynesian-type analysis to a land like India. Unemployment is chronic, largely because of the scarcity of capital and entrepreneurship. Cycles originate abroad for the most part. Consequently, deficit finance in an underdeveloped land cannot help much in creating jobs and will lead to inflation long before full employment is reached. Fiscal policy must focus on increasing investment, not because capital formation is the only thing necessary for speeding development but because it is the most important. Chelliah's interest, however, is not so much in raising collective (government) saving and investment as in expanding the *total*. The private sector, he believes, has been slighted unduly by recent Indian policy.

The role of fiscal policy should consist of "(a) increasing the rate of investment by checking actual and potential consumption; (b) encouraging the flow of investment into channels judged to be most desirable from the point of view of society; (c) in a quasi-planned economy, regulating the flow of purchasing power in accordance with the overall pattern laid down in the plan; (d) where large inequalities of income and wealth exist, modifying the distribution . . . in a manner and to the extent that are consistent with the best long-term interests of the population as a whole" (p. 52). Most of the discussion involves taxation. The general analysis of tax theory and practice seems to me excellent—resting on a broad knowledge of both Western theory and the facts of India—far-sighted and also realistic.

Taxation in India should seek to mobilize the economic surplus—not defined precisely, but income above that required for "essential consumption." The author believes that there is now enough surplus to permit nearly a doubling of investment. And the surplus will grow. The present tax system has serious defects which could be reduced by reforms which would also help mobilize the surplus. The author accepts as a datum the existence in East Asia of a powerful desire to reduce inequality, yet he is obviously less than happy about the methods now used in India. He seems to believe that this one objective should have less weight in the future than it does today for now both incentives and capital accumulation are impeded by egalitarian policies.

Agricultural incomes at present are undertaxed; unless they are taxed more effectively, an unduly large part of the growth of national income will be consumed on the farm. Personal income tax rates are too steeply progressive. (The discussion of the canon of ability to pay could be read with profit by many Americans who use the concept.) The present tax system is inequitable in that people in similar circumstances are not treated equally. The taxation of business is too heavy for an economy which needs economic expansion as badly as does India. The steeply progressive expenditures tax comes in for strong criticism; the levy reaches only about 6,000 families, is complex, and

is not necessary. Its main potential merit is that it can encourage savings. Chelliah would achieve the same goal by granting limited income tax exemptions for (new) savings (and capital gains) which are invested in forms that promise to serve the social welfare. Purchases of newly issued government bonds and newly issued shares of corporations would qualify.

Heavier use should be made of indirect taxes on mass consumption. Necessities should be exempt, but other consumption must be curbed by taxation to make resources available for investment. It is the masses, and their children, who will benefit from such forced savings. The general analysis of consumption taxes is very good indeed.

There is a role for deficit financing—but none for inflation. Within a few pages the author deals competently with these two topics. His description of the Indian revenue system and his review of developments over recent years both seem of high quality, though I am not qualified to judge them.

Both volumes reflect the efforts of professional economists to apply Anglo-Saxon economic concepts to a land vastly different from the lands where the concepts developed. Here is evidence that progressive taxation as applied in India is an obstacle to progress. The authors find Keynesian employment theory hardly the answer to India's employment problem. They implicitly reject much of our implicit theory of growth because India has too few entrepreneurs (in the private sector). On these and on other points, the authors do not always see eye to eye. Yet both see much which can help anyone who is concerned with problems of public finance in an underdeveloped land.

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The Question of Government Spending: Public Needs and Private Wants. By Francis M. Bator. New York: Harper & Brothers, 1960. Pp. xvi, 167. $3.75.

In this deceptively short book Professor Bator presents a scholarly and concise analysis of the problems involved in determining the proper allocation of resources between the public and the private sectors of the economy. By concentrating on the arguments generally used against any significant expansion in the volume of government spending, the text, which is aimed at "the persistent lay reader," undertakes to clarify the issues and to eliminate from consideration all irrelevant and incorrect propositions. The result is a lively and lucid discussion which economists will find useful for both undergraduate and graduate students. For the economists themselves there are numerous footnotes which go into the more technical aspects of the subject and which, had they been put instead into the text, would have increased the size of the book substantially.

The discussion begins, appropriately enough, with a 30-page condensation of recent quantitative fiscal history, covering the 1929-57 period. The familiar aggregate growth trends are all here, but the discussion carefully documents the extent to which they are attributable to the requirements of nation-

al defense. In 1957, for example, the ratio of nondefense government output to nondefense GNP was only 10 per cent, as compared with 12-13 per cent in 1939 and 1940 and 7.5 percent in 1929. In spite of a 50 per cent increase in real civilian output per capita between 1939 and 1957, nondefense government output per head, when converted to 1957 dollars, was the same in both years.

Two criticisms of this section may be made. The analysis throughout is based on government expenditures as shown in the national income accounts. As a result government lending and loan insurance (guaranty) programs, which have grown substantially since 1929, are omitted from consideration. In addition, some questionable comparisons are made between government transfer payments and national income, which is viewed as the sum total of incomes in the economy. From this point of view national income is objectionable because it measures private incomes before some taxes (corporate and personal income levies) but after the deduction of others (indirect business taxes). Since there is no logical support for this distinction, some other base of comparision, such as net national product, should have been used.

Having raised in Part I the possibility that government nondefense output has been neglected, and having pointed out in Chapter 5 that there is no glaring evidence that private shares have been skimped, Bator devotes the rest of the book to an analysis of the issues involved in the choice between public and private spending.

Chapter 4 examines and rejects the argument that more government spending can be obtained only be sacrificing the goal of price stability. The crucial question here is whether higher taxes will so impair work incentives that supply will contract by as much as private demand so that no resources are released for government use. To this question a twofold answer is given: (1) available empirical evidence does not indicate strong disincentive reactions to high tax rates; and (2) even if this evidence is rejected, there will surely be few taxpayers who will accept the substantial reduction in disposable income which would result from their deciding to meet the burden of higher taxes by cutting their supply of effort by as much as their demand for private output.

Chapter 6 presents the positive case for government output, based on the familiar proposition that there are certain goods and services which the private sector cannot provide and others which it will provide only in inadequate amounts (because average cost exceeds marginal cost at output levels justified by demand). In opposition, as the author notes in Chapter 7, it may be argued that governments use resources more wastefully and respond less well to changes in consumer tastes than do private markets. His reply is that even if this contention is accepted (whether or not it should be is unfortunately not considered) much government output will still be economically justified. Although government inefficiency increases the alternative opportunity costs of public programs, in other words, it need not cause their rejection unless, under ideal conditions of government operation, they would have been close to the acceptance-rejection margin.

In judging the desirability of new government projects, one should include on the negative side the real costs, if any, of the additional taxes necessitated by the programs. On this score Bator accepts uncritically the proposition that all taxes, other than lump-sum levies, distort the composition of private output, even though a number of writers, including this reviewer, have argued that excise taxes, for example, may improve, as well as worsen, resource allocation.

As a result of his examination of these and other issues, Bator concludes that what is most needed in public affairs is more widespread and better information about the economic benefits and costs of public programs, and more experimentation, based on a logical and imaginative analysis of specific cases, in place of rigid application of so-called universal rules. The significance of the book, however, lies not in these conclusions but rather in the clarification achieved as a result of a careful analysis of the various approaches, both naive and sophisticated, taken to the question of government spending.

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International Economics

Gold and the Dollar Crisis. By Robert Triffin. New Haven: Yale University Press, 1960. Pp. xiii, 195. $4.75.

Professor Triffin's latest book is a logical and timely extension of the analysis of his earlier work, *Europe and the Money Muddle* (1957). That study ended with a detailed consideration of the prospective return to convertibility, urged reliance upon strengthening regional arrangements to ease the transition and sketched suggestions for a radical reform of the International Monetary Fund. The present volume—which is a convenient assemblage of already published material—starts with the return of Western European nations to convertibility at the end of 1958, diagnoses the dangers present in an international reserve system in which national currencies play a dominant role, and develops at greater length the author's earlier proposal for a reconstitution of the IMF, complemented by augmented regional arrangements.

The technical analysis of the main part of the book, which follows a brief and lucid though simplified summary of its argument, begins with a skillful exposition of the often overlooked change in the meaning of convertibility since the happy days of the automatic gold standard. Instead of convertibility into gold, with the consequent stress on central bank liquidity, convertibility now means the right to convert one currency into other currencies, often at fluctuating rather than fixed rates of exchange. With this change in the nature of convertibility, the role of reserves is no longer primarily to maintain the convertibility of a nation's currency into gold, but to meet external deficits. Thus the test of reserve adequacy is now related, not to the liquidity of central banks, but to expected fluctuations in the balance of payments. As a convenient test of reserve adequacy, Triffin takes the ratio of gross reserves to

annual imports. On the basis of data assembled by the Fund, he finds that in 1957, the *average* level of gold and foreign exchange reserves was 35 per cent for all countries other than the United States and the United Kingdom. But the record since 1950 shows a strong likelihood of resort to severe exchange controls when the level drops below 30 per cent. Since an average of 35 per cent would naturally include some below that figure, Triffin concludes that the 1957 level was on the low side, since any decline would tend to force certain key countries to abandon convertibility at stable rates and resort to exchange instability and restrictions.

The Fund estimates that trade will expand and required reserves will grow in coming years at approximately 3 per cent per annum. Prospective increases in the gold supply would cover some 70 per cent of the additional reserves needed. Triffin is less optimistic, anticipating a growth of 4 per cent (or more); additional gold supplies would then provide only 48 per cent of required reserves. The balance would have to consist of additional holdings of the currencies of the two center countries, the United States and the United Kingdom.

This prospective increase—alongside its already large preponderance—in the role of foreign exchange, and especially of dollars, in buttressing world liquidity, is what Triffin finds profoundly disquieting. Already at the end of 1958, U.S. gold holdings exceeded short-term liabilities to foreigners by only \$5 billion. (Since then, with continued gold losses and rising foreign claims, the excess has disappeared.)

These huge claims against our gold reserves threaten the liquidity of the dollar and therewith of a large part of other countries' international reserves. They also restrict our freedom to use monetary policy to stimulate our own economic growth or to relieve unemployment. A substantial further increase in foreign dollar holdings would only make matters worse. Yet if provision is not made for increasing the reserves of the world, its rate of growth and expansion will inevitably be slowed.

The solution, according to Triffin, is not revaluation of gold, because the increase in its price would have to be steep and repeated, its benefits would be haphazard and inequitable, and to stimulate additional gold mining is a silly business. Nor do fluctuating exchange rates offer a general solution. The danger of destabilizing speculation is great, and the record of freely moving rates, as in the 1920's, is poor. Although moderate rate variations, accompanied by sensible monetary and fiscal policies, meet with qualified approval, Triffin points out that the need for more adequate reserve levels would remain. Without adequate reserves, nations experiencing balance-of-payments difficulties will be forced to resort excessively to exchange depreciation or quantitative restrictions, with consequent damage to the international payments mechanism.

Triffin's solution is to convert the IMF into a sort of world central bank. Deposits with the Fund (as with the earlier Keynes plan) would provide *international* exchange that would displace the present growing and destabilizing national currency component of international reserves. These deposits

would be made equivalent to gold and fully acceptable in world payments. The initial deposits would be established by the transfer to the Fund of present Fund balances held by members, of foreign exchange (dollar and sterling) holdings, and of gold. To ensure adequate Fund resources, Triffin would fix such initial deposits at 20 per cent of members' gross reserves and of subsequent additions thereto. Dollar and sterling holdings thus acquired the Fund could liquidate gradually, converting them into gold or deposits with the Fund itself.

Like a central bank, the Fund would be able to lend the idle reserves of surplus countries to those in need of them, or to engage in open-market operations on its own initiative in the financial markets of member countries. To avoid the inflationary bias of Keynes' proposed Clearing Union, the reconstituted Fund would be permitted to expand total international credit by its lending operations only by an agreed amount, corresponding as closely as possible to the rate of expansion of world trade.

I find Triffin's analysis cogent and compelling. The spread of the fractional reserve system in the 19th century permitted economic growth to exceed the rate of expansion of gold supplies, but also established the need for national central banks, whose deposits could serve as the reserves of commercial banks. Now, with required international reserves exceeding present or potential gold supplies, there is need for a parallel internationalization of the nongold component of these reserves. Triffin recognizes the difficulties of reaching agreement on the required institutional arrangements, and relies on an extension of regional facilities and the recent increase in Fund resources to buy time. But there is no room for complacency, and this voluume should help to dispel it.

As an alternative to Triffin's internationalized gold-exchange standard, some would prefer a system of fluctuating exchange rates, moderated by official support. Though the experience of the 1920's was unhappy, some recent examples are more encouraging. A good deal has been learned in the last thirty years; the need for relative exchange stability, and the relation between a country's monetary and fiscal policy and its balance of payments, are now widely appreciated. And though such a system would not dispense with the need for international reserves, it would reduce the amounts required, since exchange-rate fluctuations would substitute for reserve movements in the process of adjustment.

To obtain agreement on the establishment of a flexible exchange-rate system, however, would undoubtedly be more difficult than to reach accord on Triffin's proposals. The latter, though drastic, are but an extension of principles and practices with which bankers and economists are already familiar. There is good reason to believe they would work. Therefore they merit wide support. Should they fail of realization, a flexible exchange-rate system would become the most practicable alternative to a return to bilateralism and restrictionism.

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Die Europäische Wirtschaftsgemeinschaft und die Drittländer. By PAUL ERDMAN and PETER ROGGE. Basel: Kyklos; Tübingen: J. C. B. Mohr (Paul Siebeck), 1960. Pp. xii, 337. DM 28.00.

Volume 19 of the List Gesellschaft publication series returns to the topic of economic integration in Europe which was also the subject of Volumes 8 and 9. This third study is chiefly concerned with the probable effects of the establishment of the European Economic Community (EEC) on the pattern and volume of trade of the rest of the world. At a time when the tariff structure of the Common Market is the subject of deliberations among the members of the GATT, and when the eventual configuration of Europe's commercial relations depends on the outcome of the negotiations between the EEC and the European Free Trade Area, this study contributes important reference material to the discussion, especially by presenting estimates of the Common Market's import requirements from third countries over the next 10 to 15 years. Throughout the study strong emphasis is placed on the effect of demand growth within the integrated area as an offset to the price effect of the internal tariff reductions which, taken by itself, will be unfavorable to suppliers outside the Common Market.

The authors approach the subject with a theoretical chapter reviewing Scitovsky's measures of the welfare gains and losses arising from trade creation and trade diversion and analyzing the sources from which integration-induced increases in the rate of economic growth may be expected to flow. The authors agree with Harry G. Johnson that one should not anticipate much gain from increased mass production possibilities, but they expect a substantial income effect through increased investment in plant modernization, improved mobility of labor, capital, and intermediate products, and greater uniformity in the pursuit of full employment policies. Part I is completed by a survey of the effects on outsiders of the regional arrangements of the OEEC, the European Coal and Steel Community, and Benelux.

Following a discussion of the provisions of the Treaty of Rome and a survey of the present resources, production, and trade of the EEC area and of its expected economic growth, the study analyzes the prospects for exporters to the Common Market by economic sectors, by individual commodity groups, and by selected countries. It is understandable that detailed forecasts can be made only in the cases of agricultural products, raw materials, and fuels, and only for countries supplying these commodities (the country studies cover Greece, Ghana, Brazil, Nicaragua, Cuba, Pakistan, and Ceylon), while the future of trade in industrial products is much less predictable.

The authors conclude that, in view of the special provisions for agriculture in the EEC treaty, external suppliers of temperate-zone food products must expect a decline, and perhaps a complete elimination, of their sales to the integrated area, while wood, fiber, and tobacco suppliers can be more optimistic. The prospects for exporters of tropical foods are less unfavorable than those of suppliers of temperate-zone foods, although the tariff preference enjoyed by the EEC overseas territories is expected to result, in the case of these

products, in an appreciable trade diversion to the detriment of outsiders. Whereas EEC net imports in these two categories will decline over the next two decades, imports of nonagricultural raw materials and fuels, whose supply elasticities in the EEC and in the associated territories are low, will tend to rise sharply. In the area of manufactured products it is conjectured that external suppliers of consumer goods may find themselves in a less favorable position than those of capital goods.

One can hardly blame the authors for doing less than full justice to the price aspects of their investigation. The possible price developments emanating from the tariff changes and from the implied demand and supply shifts are reviewed in the theoretical chapter, but they play a subordinate role in the forecasts. It is however clear that terms-of-trade effects may qualify the conclusions reached, particularly in the case of the primary producing areas covered in the country studies.

One other point may be observed: The study rightly emphasizes the income effect of integration on imports from the rest of the world which tends to weaken or even to reverse the price effect of the tariff preference. But estimates of this income effect, as contrasted with the income effect of economic growth in the absence of integration, are not explicitly presented, although the authors allow for the integration effect in their over-all growth-rate forecast. To be sure, suppliers will be more interested in the expected change in their sales volume than in the attribution of this change to various factors. But such an attribution is necessary for a full evaluation of the effects of integration. Adding the effect of demand growth without integration to the income effect of the integration itself tends to make a regional preference system appear relatively painless for the outside world. These comments are not meant to detract from the high quality and the painstaking scholarship which make this study a useful contribution to the discussion of economic integration.

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* The opinions expressed in this review are not necessarily those of the International Monetary Fund.

Trayectoria del mercado común latinoamericano. By VICTOR L. URQUIDI. Mexico: Centro de Estudios Monetarios Latinoamericanos, 1960. Pp. 178.

This is a well-organized, intelligently written commentary on the effort which led to the Intergovernmental Conference for the establishment of a Latin American Free Trade Area held in Montevidéo, Uruguay in September 1959, and again in February 1960, out of which came the treaty creating a free-trade zone among Latin American countries and the Latin American Free Trade Association.

To be sure, not all the nations in Central and South America were represented at the conferences and eventually the Montevideo Treaty was signed by only seven of them—Argentina, Brazil, Chile, Mexico, Paraguay, Peru and

Uruguay—but inevitably other nations are bound to be attracted as the experiment develops. The United States was represented by an observer; and there was also a delegation from the International Monetary Fund.

The trade pattern of the seven signatory countries is quite different from the trade pattern of the Western European nations, the ones which were responsible for the creation of the Euromart compact. Because of this, the two efforts are only superficially analogous. To begin with, while the total foreign trade of the seven Latin American countries amounts to approximately $3.5 billion per annum (average of the last four years), trade among themselves has averaged only 10-12 per cent of that total ($350 to $400 millions). In the second place, about 70 per cent of this intratrade consists of Argentine and Brazilian exports. Furthermore, some 75 per cent of this trade is composed of primary, nonmanufactured products such as wheat, fresh fruits, coffee, edible oils, cocoa, yerba mate, petroleum, copper and timber. Finally, this small volume of trade among the countries of the area is not in balance. Peru and Paraguay have typically shown net surpluses, while Argentina, Chile and Uruguay constantly reveal net deficits in their trade accounts.

There has been considerable progress in recent years in the area toward currency convertibility and the elimination of payment restrictions. The larger part of the foreign transactions of the seven countries is now handled through free-exchange markets with uniform and fluctuating rates, but still 70 per cent of the intratrade of the area is bound by bilaterial payments arrangements. Import licensing has ceased, but Chile, Paraguay and Peru still require advance import deposits for most of their imported articles. Nevertheless, full economic rehabilitation of the countries concerned has not been completed. Their foreign exchange reserves are still insufficient and large short-term indebtedness remains to be liquidated.

The author discusses such problems as the industrial growth that the members of the free-trade area could experience as a result of the treaty; the laborious course they had to follow in order to bring their negotiations to fruition—but perhaps the most important point raised is that of the imbalance of trade among the nations of the free-trade area. This will create net area creditors and net area debtors thus emphasizing the necessity for proper financing of the enlarged surpluses and deficits which will result. All this is further complicated by the pressure of two factors: (1) that not all the nations of the area have fully convertible currencies; (2) that, due to the small volume of intratrade, some distinction should be made between trade within the area and trade outside of it.

Inevitably, two finance plans were bound to emerge. One of these plans called for the financing of trade-area deficits by outside trade surpluses. The other plan envisaged the establishment of a special area-wide payment settlements system. This second plan would make three things imperative: (a) the use of dollars (agreement dollars) as a common payments medium within the area; (b) the creation of a special clearing agency to regulate and facilitate payments; (c) the supplying of the agency with a dollar capital of its own. Peru has been the champion of the first plan, Chile of the second. Where the

dollars would come from for the proposed clearing agency is, of course, no secret to anyone.

CARLO MARIA FLUMIANI

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Introduction to the World Economy. By ARTHUR J. BROWN. New York: Rinehart & Co., 1959. Pp. 212. $3.50.

In the preface, the author writes: "The aim of this book is to introduce readers to some of the salient features and problems of the world economy and to give them some indication of the main ways in which economists set about the task of analysing them." He then, in a fashion characteristic of English writers, proceeds to cover much ground in brief compass with an informative and theoretically sound argument.

He begins with the basic elements of the economic process, including a nontechnical (*e.g.*, with no graphs or tables and few footnotes) commentary on the roles of production, consumption, investment, and saving in relation to economic activity. Then follows a concise analysis of the principles relating to pricing and distribution, a survey of the problems associated with economic growth, and finally an outline of the basic pattern of international trade. The book is policy-oriented with considerable use of factual data and an emphasis on important international, with their correlated domestic, economic problems.

This reviewer has used the text as an introductory frame of reference (followed by a more standard treatment) in an undergraduate course in international trade with good results. It contains numerous leads for further study and investigation if the reader desires to pursue certain topics with greater emphasis. For the instructor with broad interests in the international economy, the book will serve as an excellent base for outside readings with the possibility of extension in many directions. As another possibility, it would seemingly serve quite well as a one-semester introduction to economics if the subject matter was supplemented with additional readings and explanatory lectures.

This interesting and well-presented little volume by a broad-gauge economist presents the type of knowledge that is of greatest value to the policy maker. In addition, it serves admirably as a means for consolidating the thinking and filling the ever-present gaps in the knowledge of economics students. The generalizations are broad but penetrating, and must be considered with due reflection or the reader will be misled by what appears at first blush to be "easy" reading. In fact, extensive study underlies the substantive content, and the sophisticated reader should have a rewarding experience.

J. D. DEFOREST

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International Economics. By H. B. KILLOUGH and L. W. KILLOUGH. Princeton: Van Nostrand, 1960. Pp. x, 435. $6.50.

This work, the second in the Series in Business Administration and Economics edited by John R. Beishline, emphasizes the policy aspects of international economic relations. It employs considerable historical detail to illus-

trate the few principles of international economics with which it deals. The main contemporary policy issues on which it elaborates include international institutional problems, programs for underdeveloped countries, European economic integration and Russian economic competition.

The arrangement of the book is logical, althought the systematic insight characterizing the detailed discussion is somewhat impaired by irrelevant digressions. After an introductory section in which the nature of international trade is related to general notions of international economic policy, the authors tackle the theory of comparative advantage. This part of a text on international economics is perhaps the most significant test of its worth as a pedagogical instrument. If the student does not perceive the fundamental ideas of international comparative advantage and if he then remains confused over the international payments mechanism all that follows in the subject is likely to be elusive, misunderstood and evocative of unsound policy attitudes. The authors in this case follow up their analysis of comparative advantage with a section on international payments and payments-balancing operations, a subject which provides the second main test of a book of this kind. Included in this part also is a relatively short chapter on tariffs and other controls on trade. The fourth part in the book deals with postwar stabilization and development. It describes the principal international institutions such as the International Monetary Fund, International Bank for Reconstruction and Development and General Agreement on Tariffs and Trade, the attempts at building economic development in countries where little has yet occurred, the commodity stabilization schemes, and economic union in Europe. The final part examines the economic challenge to the West provided by Russian competition internationally. Having covered the components of world economic strategy, the authors turn to suggestions for future U. S. economic policy. The classified bibliography is systematic and useful.

The theory of comparative advantage is presented in an historical context. The authors employ interesting examples derived from labor productivity studies of the United Nations, International Labour Office, and Organisation for European Economic Co-operation and illustrate the factor-intensities problem with rather confusing empirical material. In their development of the theory of international trade the authors inject some very elementary discussions of the theory of the firm under competitive and noncompetitive conditions, but omit adequate development of the demand side of the theory. Their treatment therefore is mostly concerned with the production side of international economics; they thoroughly explore the reasons why high-wage high-labor productivity countries can trade with their opposites without harming living standards.

The international payments-balancing mechanism is also explained in a context of historical events. No judgment is made on the alternative exchange rate systems, but they display much confidence in the purchasing-power parity theory of exchange rates. The accounting aspects of the balance of payments, the banking and monetary connections, the role of various types of capital movements receive very little attention.

In an oddly developed argument, the Killough team feels that the ends as seen by the classical writers are suitable for the present, claiming that the principles of free-enterprise economics harmonize with goals of collective security. To realize these goals more fully, they suggest that Western economic policy should be directed towards freer international trade, more extensive, more continuous and more consistent aid to underdeveloped countries, and towards a greater recognition of national economic planning in cases in which it promotes freer trade. They argue strongly against any tendencies of the United States to retreat from advances already made on the road away from isolationism.

No direct suggestion is made by the authors as to where their book should fit in an economics curriculum. Their attempt to make detailed explanations of the most elementary economic concepts suggests that this book would suit beginning economics students. Certainly the institutional and policy discussions as well as the generous employment of historical example are very informative, although some of these tend to be too detailed and too remote to be of great value at the elementary level. The book is devoid of algebraic and graphical presentations so common even in elementary economics texts; the mathematically oriented reader will search in vain for exercises challenging his capacities. There seems to be a positive design to avoid theoretical generalities and to discount theoretical contributions useful in understanding international economics. Instead they resort to very specific examples which are well constructed in their own right.

For a study of purely international economics, the analysis evades adequate consideration of international investment, money and credit aspects of trade, international movement of factors, effects of tariffs and the distribution of the gains from trade. The authors, constantly decrying the lack of and deriding the accuracy of national-income statistics, remain very suspicious of the national-income approach to the maintenance of economic stability either nationally or internationally.

As might be expected from teamship, styles of writing within the joint product differ. This however cannot account completely for several cases of stilted, imprecise and laboriously repetitious passages. Aside from this fault, the work on the whole offers a rather refreshing and competitive approach to the general subject of international economics, although there is a good case for differentiating its title from the many others with identical designations which have appeared in the past decade.

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Business Organization; Managerial Economics; Marketing; Accounting

Marginal Aspects of Management Practices. By FREDERIC N. FIRESTONE. East Lansing: Michigan State University, 1960. Pp. xii, 80.

This short book, an adaptation of a doctoral dissertation, is the most recent of a series of studies initiated by James S. Earley to test the significance of

the "marginalist approach" to the theory of the firm.[1] The author concludes, on the basis of responses to two questionnaires sent to approximately 100 "excellently managed" companies, that (1) firms using market analysis extensively in decision-making respond with generally "marginalist attitudes" to questions regarding pricing, marketing and new product policies; (2) the extent to which the selling function is represented in decision-making is significantly correlated with marginalist attitudes; and (3) the extensive use of modern budgeting techniques involving frequent revision is suggestive of a marginalist approach.

These conclusions are based upon statistical correlations for which "the significance of a chi-square test is at the 5 percent level or better." By combining answers to different questions, the author has created various indexes, such as an "index of market analysis," a "full costing index," a "market awareness" index, a "new product index," and a "quality of budgeting" index. These and others are correlated with each other and with responses to particular questions.

The care with which the statistical manipulations have been carried out is impressive, but this reviewer wonders whether such care is warranted given the probable interdependence of the responses. For example, if the respondents were at all concerned with the internal consistency of their answers despite actual procedures, much of the apparent correlation among these answers would be spurious; further, while many of the correlations relate responses in one questionnaire to responses in another, the responses come from the same set of companies; finally, some of the indexes constructed contain common questions.

Some interesting results do not depend on these correlations. Apparently these "excellently managed" companies do not rely heavily on cost-plus pricing. Sixty-one per cent of the respondents regard market analysis as more important than accounting or engineering analysis in decisions involving price changes; 62 per cent regard market analysis as most important in the pricing of new products; and 77 per cent agree that it is most important in fixing product prices in different areas.

Data on the participation of sales personnel in the making of various kinds of decisions is also illuminating. Sales representatives participate in decisions on product pricing in 100 per cent of the reporting firms; 99 per cent of these firms use sales people in making decisions on the introduction of new products; sales participation in decisions on research programs is utilized by 87 per cent of the firms; and in making decisions on changing production methods 71 per cent of the firms include sales personnel. Against this background, it is surprising, however, that only 44 per cent of these "excellently managed" companies involve sales representatives in making investment decisions. In this area the major burden falls on accounting and finance personnel.

Unfortunately the study never makes fully clear just what it is that is being

[1] Other published studies include James S. Earley, "Recent Developments in Cost Accounting and the 'Marginal Analysis,'" *Journal of Political Economy*, LXIII (June, 1955), pp. 227-242 and his "Marginal Policies of 'Excellently Managed' Companies," *American Economic Review*, XLVI (March, 1956), pp. 44-70.

tested. "Marginalist attitudes" are frequently referred to but never adequately defined, and the word "marginalist" appears as often in quotes as not. Whatever the intent, the study does reveal that "excellently managed" firms are aware of variables which affect profit and consider data on them in making decisions. The more ambitious questions, "Do these firms attempt to maximize?" and "Do they employ marginalist calculations in the attempt?" cannot be settled on the evidence offered here.

EDGAR O. EDWARDS

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Ekonomika sovetskoi torgovli. Uchebnoe posobie. (The Economics of Soviet Trade. A Textbook.) Edited by S. V. SEREBRYAKOV. Moscow: Gospolitizdat, 1959. Pp. 479. Rbl. 9.

The dozen staff-members of the Plekhanov Economic Institute in Moscow who have contributed as many chapters to this work have produced less the textbook on the economics of trade which the title page promises than a symposium on Soviet internal-trade arrangements. All but the first chapter are essentially descriptions of management techniques rather than exercises in economics; and Professor Lifits disappoints readers of his previous writings in the opening contribution entitled "Soviet Trade and Its Role in the System of the U.S.S.R. National Economy," intended to provide the theoretical setting. The preparation of an index could, moreover, not only have enhanced the claim to be a textbook but might have moved the editor to reduce the tedious repetition and overlap between chapters.

The description of planning procedures in this sector is probably the most interesting feature of the book and is chiefly to be found in Fefilov's chapter on plan methods and Genkina's on retailing. The key figure is Gosplan's estimate of household incomes, which define the retail turnover needed to clear the market (over 80 per cent of the State Bank's cash receipts derive from retail sales, and no salaries are paid by check). Since output balances for consumers' goods—whence, by deduction of supplies to organizations, are derived retail availabilities—are drawn up, in physical and then value terms, only for a restricted number of important commodities, there seems to be some element of estimation in compiling planned retail sales. The process of bringing sales and purchasing power into the desired equilibrium is not precisely described either in these chapters or in Kulikov's on price formation, although there is an interesting prospect of iteration in the practice of planning wages of retail staffs as a percentage of trade turnover.

The book's main emphasis is on the disaggregation of these global balances down to the level of the shop or restaurant. In this the contributors take especial care to define every term used, and thereby enlighten users of Soviet trade journals (*e.g.*, in defining "transit sales"—direct sales from factory to shop), and statistics (*e.g.*, "nonmarket funds"—sales of nonfoodstuffs to institutions—or in stating retail turnover to cover sales of standing timber in state forests).

They also put forward much good advice to trade organizations, ranging from the economics of self-service in supermarkets and cafeterias to the size

of deliveries (women form 72 per cent of trade workers and are not permitted to lift loads exceeding 20 kg., but deliveries by wholesalers are often in 100 kg. sacks or 200 kg. bales) and a wealth of facts not elsewhere readily available (*e.g.*, that income in kind is 5 to 6 per cent for a nonfarm worker against one-third for a collective farmer; on the postwar rise, now past its peak, in street vendors as a proportion of total retail employees; on the smallness of regional wage-spreads—16 per cent maximum—and of trading margins—a maximum of 3 per cent in wholesaling and an average of 6½ per cent in retailing). However only Gogol, on manpower and wages, Smotrina, on overheads, and Kuzin, on accounting, illustrate their narratives with factual statistics, mostly 1957 and 1958 data; the others use notional examples, of which an improbable, if trivial, case is that showing, for restaurant input-planning, as much meat (76 gr.) in potato soup as in *borshch*.

There are some criticisms of the retailing system of the present or the recent past, but most of them no more than echo recent party and government resolutions; *e.g.*, Dneprovsky on low agricultural prices, Vasilev on shortcomings in the catering services, and others on the overcentralization of trade planning and pricing.

At two points, however, the criticisms appear original. Kistanov attacks the dispersion and overlapping of trading agencies, despite the mergers and concentration of authority in Union-Republic Ministries of Trade since 1957 (it may be computed from figures in the book that nearly one-third of 1958 turnover took place in "closed" shops, mostly run by *sovnarkhozy* in their factories, but some by GUTMO—the Soviet army PX). Kulikov finds irrationalities in price zoning and—following earlier criticisms in the press—disapproves of the 7 per cent surcharge on rural sales (saying that it leads to peasants wasting time in journeying to urban shops and to "speculation") but Smotrina defends it because rural stock and transport charges are higher.

While the Western reader may disagree with most, if not all, the assertions about capitalist trade, he will be most surprised by the absence of any but the simplest statistical techniques both in the analysis and in the reported practice of Soviet trade planners. The most that Genkina can say about inventories is that they are a positive function of turnover (in fact the behavior of Soviet trade stocks would repay study, for—far from expanding, say, in proportion to the square root of the turnover increment—they have risen *faster* than turnover).

Serebryakov mentions that wholesalers are responsible for quality control but does not describe sampling methods. Pirogov on fixed assets spends much time defining the notional capacity of soup cauldrons but almost nothing on capital efficiency. Despite the immense hauls for some consumer goods (averages of 2,000 and 1,350 km. are cited for canned goods and cigarettes), the only "programming technique" described by Serebryakov to choose supply and outlet points is visual inspection of a "chessboard tabulation" of those points. In numerical examples the elasticity of demand with respect to price is everywhere assumed to be infinite and the beginning of more refined Soviet work on consumer demand (notably on income elasticities) is ignored. Moreover the alternative set of demand signals—stock fluctuations and counter

assistants' records of customers' unfulfilled enquiries—often stressed by other Eastern European trade economists, is also scantily treated.

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Critique économique du prix de revient industriel. By JEAN-PIERRE DE BODT. Louvain and Paris: Nauwelaerts, 1959. Pp. xiv, 178.

The *Critique* is a straightforward, if pedestrian, attempt to show that conventional cost accounting techniques (on the Continent, at least) stand in need of marginal analysis. The author is a Belgian business executive as well as teacher of theory at L'Institut Superior de Commerce St. Ignace at Antwerp. His purpose is to provide management with more precise and rational criteria with which to pursue its goal of profit maximization, and the result is largely indebted (with acknowledgement) to the presentations of Dean and Earley in the United States. The work therefore offers little to U.S. economists and those active in U.S. management who are interested in the application of marginalist concepts to corporate direction. Apparently marginal cost accounting is not widespread in Belgium.

The level of treatment is commendably simple, but the content scarcely surpasses that of a good undergraduate microtheory course in a school of business administration. In the third and final chapter there is a case of actual application of marginal analysis in a Belgian textile concern, which shows admirably the relevance to such questions as the introduction of new lines, the dropping of old products and the control of costs of various plant divisions.

The author is well aware of the pragmatic difficulties in obtaining precise marginal costs and recognizes that the cost accountants' figures are typically "*average* marginal costs." He stresses the need to distinguish carefully between general fixed costs and those portions of fixed costs which can be precisely attributed to separate divisions or products.

There is clear recognition too that the essential problems of the management of the firm cannot be met simply with given technique of cost accounting, that such tools must be used with care and flexibility and that there is no substitute for business acumen. The book should be a good introduction to the subject for European management, which generally has somewhat less university and economics training than is the case here. It should be noted however that the application of marginal accounting in the United States is by no means widespread nor is it necessarily sure to prevail.

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Industrial Organization; Government and Business; Industry Studies

Concentration in British Industry. By RICHARD EVELY and I. M. D. LITTLE. New York: Cambridge University Press, 1960. Pp. xvi, 357. $10.00.

This National Institute of Economic and Social Research study includes

analyses of British concentration statistics for 1951 and of changes in concentration from 1935 to 1951; and a review, based chiefly on published sources, of developments in 20 industries since 1935. The statistical source is the Census of Production, from which the Board of Trade made available the 1951 totals for establishments, gross and net output, and employment of the three leading "units" (companies or company groups) in each trade.[1] The 1935 data are those presented by H. Leak and A. Maizels in "The Structure of British Industry," *Journal of the Royal Statistical Society*, 1945, Vol. 108, parts 1-2, pp. 142-207.

In 50 trades, the 3 largest units accounted for 67-100 per cent of either employment or net output in 1951; in 69, they accounted for 34-66 per cent; in 101, for 33 per cent or less. These high-, medium- and low-concentration groups employed 10, 24 and 66 per cent, respectively, of the total labor force. Manufacturing concentration in the United States, based on value of shipments in place of employment and a 4-company instead of a 3-company concentration ratio, shows a similiar distribution.[2] It has been previously found that 4-company concentration is higher for most industries in Canada than in the United States [2, p. 76].

Another classification employed by Evely and Little may be condensed as follows: 84 trades contained big concerns, many of which "dominated" their trades or were price leaders; in 61, the firms were about equal in size, but so few in number that the situation was "favorable for collusion" (p. 10); and the conditions in 74 were "probably close to those of perfect competition provided that there is no collective regulation"—a significant qualification (p. 11).

Among the findings on plant size is the more frequent association of concentration with relative plant size than with relative number of plants in a unit, although technology appears to bar small plants in only 26 trades. As to why "there has been a marked tendency for high concentration, once attained, to persist" (p. 130), the report points out that newcomers shun a trade which is contracting, while various factors related to economies of size (and sometimes private or government restrictions) hamper entry into some expanding trades.

One problem in handling the material is that "a considerable part of the variations in concentration is inexplicable in terms of measurable economic facts" (p. 21). Different trades do not respond in the same way to similar forces. Another problem is the limited significance of some statistical measures. Thus the $-.83$ correlation of concentration ratios and number of units (introduced with a misprint, "lower" for "higher," p. 14) "is hardly surprising," since "low concentration cannot occur with few units" (p. 105). The

[1] For 23 trades the Board of Trade, applying the more cautious U.S. Census Bureau policy on company disclosure, used 4-company totals; for 13 trades it used 5 or more.

[2] The 62 4-digit industries with 67-100 per cent concentration in 1954 accounted for 11 per cent of value of shipments; the 171 with 34-66 per cent, for 26 per cent; and the 201 with 33 per cent or less, for 63 per cent (computed from [1, pp. 23, 243-65]). Comparability would require that certain large nonmanufacturing trades such as laundry work and construction, included in the British totals (and whose local-market character blurs the meaning of the national concentration statistics), be added to the low-concentration bracket in the United States.

.84 correlation of concentration ratios and size ratios (average employment of the three largest units divided by average employment of all units) is still less surprising. The negative correlation (−.40) between number of units and size ratios, is hard to interpret—and, in fact, when size ratios are defined in terms of *other* instead of *all* units, it becomes a weak positive (.16).

No over-all measure of concentration change from 1935 to 1951 is given, but a median of 31 in 1935 and of 37 in 1951 can be derived from the gross output concentration ratios of 185 trades (Appendix J). With their usual caution, the authors isolate only 27 trades where "principal product concentration" clearly increased, and 14 where it decreased. From the chapter reviewing the growth of concentration in 14 of the 27 trades, the reader may infer that 6 (cans, lead, oil refining, razors, soap, watches and clocks) increased their concentration as a result of commercial, financial and technological advantages of size, with acquisitions playing a very minor role. Coal nationalization was largely responsible for increased concentration in 2 trades often operated by coal companies (bricks, coke ovens); and government-encouraged amalgamation and quotas were controlling in another (sugar). In 5 trades (baking, film, metal mining, tinplate, wrought iron and steel tubes) acquisitions, to achieve self-sufficiency and to take advantage of modern large-scale technology, were important. Concentration, in brief, resulted from technology, mergers and government action.

The report is packed with data of varying degrees of importance, but all of it worth gathering so that no clue to the understanding of British concentration will be missed. Its value to most of those consulting it will be in the historical review and the newly published figures for individual trades. Only a very few specialists can undertake the long, hard labor of reviewing the statistical analysis. Students will regret that such a masterly analysis of complex material should have to wait until nine years after the period represented by the data.

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References

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Proceedings, International Conference on Control of Restrictive Business Practices. Glencoe, Ill.: The Free Press, for the Graduate School of Business, University of Chicago, 1960. Pp. xix, 380. $10.00.

The international conference on control of restrictive business practices was convened in Chicago in mid-January 1958 under the auspices of the Graduate School of Business, University of Chicago with financial support from the Ford Foundation. In attendance were 64 government officials and private persons, all in an individual rather than an official capacity, from 16

countries. What are best characterized as the minutes of the meetings are contained in the volume under review.

The book itself is divided into four parts: (a) public lectures describing the law with respect to restrictive agreements and the procedures by which the law is enforced in the United Kingdom, the Netherlands, Norway and West Germany, (b) prepared statements on national policy towards such arrangements in Austria, Belgium, Canada, Denmark, France, Ireland, Japan and Sweden, (c) summaries of discussions at closed sessions among and between the participating governmental and private groups, and (d) texts of major legislative enactments concerning restrictive practices in various nations since 1954.

To summarize a document of this kind in a few well-chosen statements is a demanding, perhaps impossible, task. The discussants touched on a great many specific issues, they tended to stress the differences rather than the similarities in their points of view, and—to judge from the summaries of their conversations—frequently talked at cross-purposes.

Nonetheless, a few generalizations seem warranted. For one thing, there was broad, though not unanimous, agreement among those present that the preservation of market rivalry tends to promote the public interest (somehow defined). For another, there was consensus that public sanctions should be invoked only against those restraints of trade which are unreasonable; there was but limited support for per se rules. Finally, it was broadly agreed that, in view of the difficulties in assessing the economic effects of a given restrictive agreement, law-enforcement officers must be rugged empiricists.

As these summary observations should indicate, the conferees trod on familiar paths. From their point of view, no doubt, the exchanges of information and ideas proved valuable. The same cannot be said for the nonparticipant forced to rely on the proceedings of the conference. Not only does the published record reveal little that is new or startling, but it lacks the full flavor of what must have been vigorous interchanges among knowledgeable persons. To put it directly, the colloquies appear unmistakably to have been strained through the fine mesh of the four *rapporteurs*, losing in the process both body and bouquet.

For all this, the record of the conference is of some value to specialists and nonspecialists alike. It constitutes a handy guide to cartel law and its administration in a large number of nations. It contains a fairly extensive, even if none too profound, treatment of differences in national policies toward restrictive agreements, together with the reasons therefor. And it re-emphasizes in a variety of ways the impossibility of molding diverse national economies in the image of a single, "universal" theoretical model.

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Antitrust in the Motion Picture Industry: Economic and Legal Analysis. By MICHAEL CONANT. Berkeley: University of California Press, 1960. Pp. ix, 240. $5.50.

Utilizing the antitrust cases in the motion picture industry, the voluminous

record of the Paramount case (United States v. Paramount Pictures, 334 U.S. 131 [1948]), the critical literature in economic and legal journals, congressional investigations, and fifteen years experience with the Paramount decrees, Michael Conant has made an analysis of superior quality. It goes far beyond the limited scope of earlier works to concentrate on the economic consequences of a tightly controlled pattern of systematic discrimination. Among empirical studies of discrimination it ranks with the best.

Monopoly power in the industry did not emerge originally from control of supply. To be sure, certain institutional factors—copyrights, patents, preclusive control of personnel, censorship—provided the potential means for controlling supply, but, as Conant puts it (p. 39): "monopoly power sufficient to control supply could be enforced only at the exhibition level." Producers had to establish market control if they were to achieve monopoly power. This compelling necessity dictated the grand strategy of the industry. The implementation of this strategy resulted in one of the most perfect patterns of discrimination ever devised.

The key to the system was ownership of first-run theaters by the Big Five producers. This attained, they established a complicated, rigid pattern of discrimination in respect to licensing runs, clearances, zoning and admission prices—all directed toward maximization of profit. The rules of the game were ruthlessly enforced by local Film Boards of Trade, dominated by the Big Five. Independent producers were excluded from the market; independent exhibitors were relegated to the bottom of the market hierarchy and forced, by block booking, to take low-grade, unwanted films in order to get any good ones. Three small firms, who owned no theaters, were dragooned into joining the combination. The Big Five compromised their oligopolistic rivalries by market-sharing, interchange of films and pooling of profits. The federal government, for some twenty years (1926-45), not only condoned, but "fostered and abetted" this monopolistic arrangement.

The Paramount decision broke the back of the motion picture cartel. In 1945 there were only 40 small, independent producers, who made less than 5 per cent of the class A films; by 1957 there were 165 independent producers, who made more than 50 per cent of the class A films. In Conant's own words (p. 219):

> The decrees of divorcement and divestiture effectively put an end to the organized control of the industry. . . . The major's near monopoly of first-run theaters was broken. . . . Subsequent-run theaters were relieved of the oppressive restrictions of block booking. Divorcement also curtailed the monopoly power of the five majors on the distribution level. Other distributors and independent producers were given freer entry to compete in the market for screen time.

The Paramount structural changes, however, failed to achieve perfect competition. The Big Five still dominated production and distribution. The divestiture of 1200 theaters by the Big Five left in existence 5 formidable theater circuits concentrated, as before, in particular areas. The old pattern of discrimination and price fixing remained substantially intact. Conant is se-

verely critical of the Court's failure to reorganize the marketing system. He feels that the large theater circuits should have been fragmented, competitive public auction-bidding for films made compulsory, clearance agreements prohibited and dead time between runs eliminated. Exhibitors continue to complain about discrimination and excessive film rentals, and advocate either compulsory arbitration or public utility regulation for their protection. The author has no confidence in either of these prescriptions; he would rely on the widest possible dispersal of market power and free entry of rivals into the market.

The admirable quality of the author's empirical research is somewhat marred by his equivocal conclusions about discrimination and public policy relating to it. The nature of the product and the market, he thinks, makes some pattern of discrimination necessary. Since neither production nor exhibition is, or can be, perfectly competitive "public policy requires that distributors be free to adopt the pattern of release and rental discrimination that they consider optimum" (p. 215). Otherwise, he assumes, production may decline, rental and admission prices rise, some theaters close, and people see fewer pictures.

There is a contradiction here between the analysis and the policy prescription. Conant criticizes the Paramount decrees for their failure to make the market more competitive. His own recommendations for market reoganization, if adopted, would reduce discrimination to minimal proportions. He is a staunch advocate of free competition, dispersal of market power and freedom of entry. These antimonopoly views are strangely inconsistent with the conclusion that distributors should be free to discriminate. Discrimination can scarcely be justified by the supposition that untoward economic consequences for beneficiaries might result from its demise. This is the price that must be paid for economic freedom.

Horace M. Gray

University of Illinois

Government Promotion of American Canals and Railroads, 1800-1890. By Carter Goodrich. New York: Columbia University Press, 1960. Pp. viii, 382. $7.50.

In this book Carter Goodrich presents a well-ordered and comprehensive review of the range of public support in varying forms that went into building the major heavy-duty transport system of nineteenth century America. In addition to treating the better-known examples of federal and state action—the early plans, the Erie Canal, the Pennsylvania State Works, the transcontinentals, etc.—the author has brought together the results of recent research into the extensive activities in which federal, state, and local governments engaged in all parts of the United States, both before and after the Civil War. After an admirable short chapter setting the stage, temper, and tempo of the times, the book examines the scope of federal policy and action for each period, and then devotes more than half its pages to the subsequent "Emulation of States and Cities" in the same undertaking.

The survey drives home on a broad front what had been established for more limited sectors by a growing roster of monographs, many under Goodrich's direction: that the basic developmental transportation of this country, like comparable social overhead elsewhere, came into being when and where it did only by massive public assistance. To open new territories and accelerate the exploitation of their potentials, nineteenth century Americans readily turned to public treasuries for tasks too large for private capital.

What was distinctive about U.S. patterns, Goodrich emphasizes, was not the use of public funds, but other characteristics: First, the pluralism and pragmatism of the effort, involving decision and extensive financing at local and state levels as well as federal and private, with any and all sources of funds operating through enterprises that were usually private, often mixed, and sometimes public. A second feature was the extent to which the exploitation of the resulting opportunities was left to the spontaneous individual decisions of private interests, and the speed and vitality of these responses. Once the cogent program Gallatin offered in 1808 had been abandoned, there was almost no centralized planning of transport development; and there was virtually no planning at all of the actual flow of people and capital into the agricultural and complementary activities that transport allowed. The record reveals instead a rich, gaudy, wasteful, multicentered scramble by which for the first time the resources of a continent were successfully brought into play in two or three generations.

The story is a remarkable epic by any standards. It runs from the project of the National Road (1806), through the early visions of Washington, Gallatin, and J. Q. Adams, to the dominance of sectional interest and later of private finance. It includes early canals projected over mountains, the attempts to use horses and even sail on the pioneering B&O railroad, the promotional conviction of Asa Whitney in the 1850's that a transcontinental railroad would make Europe "bow to Asia, and Asia to Europe, across our bosom." At a deeper level it raises lasting issues of public policy in the use of resources and the regulation of enterprise. By posing the question of what *would* have been the condition of transportation at the mid-century (or even in 1890) without public funds, it suggests another question on which we all can speculate: Whether the Union would have remained one if railroads in the decade before the Civil War had not provided east-west transportation possibilities to large areas of the Ohio and Mississippi valleys that until then had been forced to depend on a southern outlet through New Orleans?

The book pursues no such speculations. Its strength is in its orderly presentation of the known, in the completeness of its coverage of the much less known regional activities, and in the good sense of its evaluation of the problematical. The historian will miss the color and specifics of particular ventures and the ways in which given transportation developments affected given communities—necessarily excluded by the scope of the work—and maps which should have been provided beyond the single one offered. The economist will wish the book had done more to relate magnitudes of investment in these facilities to the prevailing levels of total investment. Direct and indirect effects of transportation expenditure on the business cycle and on the structure and

trend of the economy get spare mention, and the changing technology virtually none. The focus is rather on the locus of funds and decision-making and on the politico-economic instruments of action through which they moved. Of this it provides an extremely useful synthesis, with judicious comparative observations on procedures in other nations and how far this experience is or is not relevant for underdeveloped economies today.

JOHN E. SAWYER

Yale University

Problems of Public Enterprise—Thoughts on British Experience. By B. V. V. RAMANADHAM. Chicago: Quadrangle Books, Inc. and London: Frank Cass & Co., 1959. Pp. 176. $4.75.

The author of this book, first published in London under the title *Public Enterprise in Britain,* has observed at first hand the working of public enterprise in England, which can be considered an experiment by the rest of the free world. He has coupled objectivity with acute economic perception, and has produced a clear and penetrating study of the working of nationalization in industry.

The book does not attempt to argue the case for or against nationalization. Rather, on the assumption that some nationalization is already here and is likely to remain, and more may be forthcoming, the author discusses a number of important problems of nationalization including organizational arrangements, management, accountability, pricing, resource allocation, capital financing, subsidies, consumer representation, and public control. The book is addressed to both economists and public administrators in the hope that it may be used to obtain the most satisfactory results possible under nationalization. No attempt is made to cover all aspects of nationalization nor to present full studies of individual nationalized industries, except in part, electric power.

A careful study of recent British experience in the field of public enterprise has much to contribute toward better understanding of the problems and difficulties of a public sector gradually being enlarged on economic (efficiency) rather than political grounds. The British scene offers advantages of considerable homogeneity inasmuch as all of the recently nationalized industries have been given the public corporation form of organization, and all resulted from the taking over of existing private enterprises. In India, in contrast, certain nationalized industries were created initially by the central government.

The public corporation as an institutional form of industrial organization is here to stay. It is defined as a nationalized industry which is not organized as a governmental department nor run by a local authority nor given joint stock company form, and which operates on the principle of financial self-support. It has private status and ability combined with public purpose and responsibility. An example in this country is the TVA. The book attempts to work out a norm or optimum toward which the public corporation should strive if it is to achieve all its basic objectives in the most satisfactory and efficient manner.

One pioneering feature of the book is a discussion of the role of the public corporation in an underdeveloped nation, in which managerial leadership is a major problem. In the case of Britain the same management largely was used

under public operation as under private operation of industry. This has not been true of India and other countries. It is the author's observation, supported by American experience, that successful private businessmen are not best suited always to appointment to the boards of nationalized industries. Thus civil service in certain countries has a major problem on its hands.

The autonomy of public corporations in an underdeveloped nation must be qualified to counter not only regional inequalities but also those among broadly distinguishable consumer groups. The implementation of social policy objectives is not incompatible with corporate autonomy providing certain limitations on social policy are written into the basic Acts. The success of public enterprise always depends upon a proper balance between economic and political considerations.

In the case of cross-subsidization such as in the rural areas of North Scotland, the author argues that the taxpayers in general should pay any subsidies involved and not particular power users, usually urban consumers. The optimum economic unit is achieved at the lowest point of the average total cost curve. The optimum managerial unit is exceeded if expansion to promote rural electrification results in higher rates to urban groups. There is no incompatibility, however, in different prices to different consumer groups so long as the prices charged are less than the cost of alternative sources of the same product or service. This is the discrimination case as contrasted with the cross-subsidization case.

One of the advantages of nationalization is that it can remove certain market imperfections and at the same time achieve certain external economies. It does not, however, automatically solve all problems of monopoly. The profit motive is gone, to be sure, but questions of high costs through inefficiency, prices unrelated to costs, and nonprice related input-output problems remain.

This little book is well worth reading, and it makes a valuable contribution to the economic and public administration literature.

E. K. Zingler

University of Houston

Ferrocarriles. By Carlos Villafuerte. Mexico, D.F.: Fondo de Cultura Economica, 1959. Pp. xix, 281.

This volume is an excellent study of the present condition of the Mexican railroads. It is part of a large-scale study of "the economic and social structure of Mexico" sponsored by the Mexican government's development bank Nacional Financiera. Unlike some other volumes in the series, this one is exceedingly well done. It is written with an objectivity and forthrightness that is a tribute to Mexican scholarship.

The author is fully aware of the sad shape into which the Mexican railroads have been allowed to fall. This decline has come as the result of the long process of the Revolution, and the concentration of successive governments on problems which have seemed to them more pressing. However, he is moderately optimistic about the possibility of rebuilding the railroad system and of their continuing to play a key role in the nation's economy.

The volume discusses many aspects of the functioning of the country's railroad system, including labor force, capital equipment, and technical and financial resources. It also describes at length the actual state of the various lines in the Mexican system; and discusses the modernization program underway during the last half-dozen years. This program, partly financed by foreign loans, includes not only improvement of existing lines and the extension of the system, but also extensive renovation of the rolling stock. The author presents his assessment of the importance of various improvements being undertaken.

The last part of the book is a discussion of the problems facing the Mexican railroad system. Many of these would make familiar reading to anyone in the least familiar with the state of U.S. railroads today. One basic obstacle to improved efficiency of the Mexican roads lies in the strong position of the railway workers union. This organization, like its northern counterparts, is regarded by the author as likely to be strongly opposed to any move which would reduce employment on the railroads. He suggests the need for close consultation with them in making out programs for increasing the utility of the railway system.

Another basic problem is the competition between the railroad system and the growing highway network. The author points out that the railways have been carrying a steadily smaller percentage of the country's freight and passenger traffic, due largely to this competition. However, he does not think that this decline necessarily need continue. He points out that highway traffic has certain disadvantages, such as the growing amounts of equipment and fuel which must be imported from aboard. He presents figures to show that if the railroads can improve their efficiency to the degree foreseen as a result of the modernization program, they can compete economically for many kinds of traffic.

However, the author does suggest certain changes in present-day procedures. He advocates coordination of government roadbuilding with railway construction and renovation projects, so as to provide feeder roads for the railways as often as possible and competitive highways as infrequently as possible. He also favors a revision of the railroad rate schedules, changing their base and increasing some tariffs, so as to make the railroads as nearly self-sustaining as possible.

Another problem reminiscent of the situation in the United States is that of integrating the whole nation's railroad system. It is not as complicated in Mexico as north of the border, since virtually all roads are owned by the government. However, the author sees a need for establishing a unified rail network, and as a starting point, the establishment of several well-delineated regional systems. He foresees considerable possible savings and increased efficiency in unified repair facilities, as well as in the establishment of uniform rate schedules and by facilitating train shipment of goods over more than one of the existing railroad systems.

Other suggestions of the author are peculiar to Mexico. For instance, he feels that the country's railroad system would benefit considerably from an

increase in the capacity of the nation's steel industry. This would make it possible to produce a much larger proportion of rolling stock and other equipment in the country, thus saving on foreign exchange. He is also concerned with the role of the railroads in "opening up" parts of the country at present virtually outside of the national economy, and believes that the establishment of such lines should be decided upon the basis of broader national economic considerations rather than merely the finances of the railroads themselves. Finally, he suggests the need for a much more ample official survey of the Mexican railroads, to be used as the basis for future planning of the country's network.

This book is a very worth-while contribution to an understanding of the economy of present-day Mexico. As the first over-all study of the Mexican railroads, it should be useful not only to those concerned with the future development of the Mexican economy, but to foreign students of the subject as well.

ROBERT J. ALEXANDER

Rutgers, The State University

Labor Economics

The CIO Challenge to the AFL. By WALTER GALENSON. Cambridge: Harvard University Press, 1960. Pp. xix, 732. $9.75.

This volume, part of the Harvard project on labor-management history in recent decades, covers developments in the labor movement between 1935 and 1941. More specifically, the author deals with the genesis of the CIO, with the organization of the steel, automobile, and other mass-production industries, with the labor thrust in coal mining, clothing, meat, lumber, petroleum, the maritime industry, with the Teamsters, the Machinists, the building trades, the printing and publishing unions, and the railroad labor organizations. And there is a final chapter which focuses on some general dimensions of the labor movement during this period—union membership, union finances, work stoppages, political activity, legislation, etc.

To survey accurately and constructively so vast a field in approximately 700 pages is a formidable task indeed. But Galenson has for the most part succeeded admirably. He has culled from the welter of available material the salient features of each problem and described them with a striking economy of language. As one might expect, some chapters are more detailed than others. Thus, for example, those on the Machinists and the printing and publishing unions seem somewhat sketchy to this reviewer. But perhaps such variation in coverage was unavoidable, given the absence of equally numerous dramatic and important events in the various unions on the one hand, and the lack of relevant informational sources for some of the developments in certain unions on the other.

The volume is essentially a descriptive one; and the description, it should be hastily added, is both dispassionate and judicious. Here and there Galenson does make brief forays into analysis. And it is precisely because these forays

reflect such keen insight that it is regrettable he did not dig more intensively and extensively into the determinants accounting for the developments which he has described so well. To be sure, a good deal of such analysis would, of necessity, have assumed the character of hypothesis rather than proven theory. But hypotheses coming from someone of Galenson's competence who has dug so carefully into the "raw material" would have been more than just desirable; it would have proven extremely useful to other researchers. For instance, in dealing with union membership data from 1935 to 1941, the author makes no attempt to specify the determing factors and their interaction which were responsible for shaping the pattern of union growth during those years. Yet such a theoretical stab by one so familiar with the period would have proven most suggestive to those scholars who are groping for a general theory of union growth.

Despite the inadequacy of the analysis, however, this book is a first-rate piece of scholarship that provides the reader with a vivid and meaningful picture of developments during those dramatic six years which were truly one of the major turning points in the orientation of the U.S. labor movement. It is an excellent descriptive synthesis of the period, which was sorely needed. And Galenson is to be highly commended for filling so important a gap in the literature in so competent a manner.

JOSEPH SHISTER

University of Buffalo

Wages and Earnings in the United States 1860-1890. By CLARENCE D. LONG. Princeton: Princeton University Press, for the National Bureau of Economic Research, 1960. Pp. xvii, 169. $4.00.

American industrial growth began in earnest in the decades that followed the Civil War. Pig iron production rose from under a million tons in 1860 to 10 million tons in 1890. Employment in manufacturing and construction tripled. An overwhelmingly agrarian nation in 1860, the United States was vying with Great Britain for industrial supremacy thirty years later.

It is with this critical period in our development, one that deserves much closer study than it has received for the light it can throw on the problems of economic growth, that Professor Long is concerned. Using the same basic data as other investigators before him, those contained in the Aldrich Report, the Weeks Report, and Bulletin 18 of the Department of Labor, but employing different averaging and weighting procedures, he finds that money wages rose by about 50 per cent from 1860 to 1890. This is substantially less than the indicated increase in the index prepared by Wesley C. Mitchell, heretofore the standard wage index for the period.

To deflate the money-wage series, Long uses, for the years 1860-1880, a new cost-of-living index prepared by Ethel D. Hoover for the National Bureau of Economic Research. For the following decade, Long has worked up his own index in place of the Burgess index, the only one available up to now. On the basis of the revised data, it appears that living costs were almost the same in 1860 and 1890, so that the advances in money and real wages were identical, that is, about 50 per cent. A substantial part of the increase came in the third

decade as a result of an increase in money wages (15 per cent) and a decline in the cost of living (11 per cent).

If there were any criticism to be made of this impeccable study, it would involve Long's cost-of-living index, so crucial to the novelty of his findings. Clearly superior to the old Burgess index, it is nevertheless based upon a thin sample of price data. Long himself says with candor: "How accurately this new index measures the cost of living is probably impossible to say . . . it is undoubtedly inferior to modern indexes, and could surely be improved by an exhaustive investigation of newspaper advertisements, store catalogues, and business and family records."

The latter part of the study is concerned with an analysis of wage structure. And in conclusion, Long asks: why was the annual real wage advance from 1860 to 1890 only 1.6 per cent, a tempo of "allegretto rather than allegro?" The answer is couched in terms of the rapid growth of the industrial labor force due to immigration and movement out of agriculture, the drawing off of available resources for heavy capital investment, and the absence of strong trade unions. Even in as richly endowed a nation as the United States, a relatively austere period of capital formation had to precede expanding consumption.

This monograph represents the method of quantitative historical analysis at its best. The statistics are set forth clearly, and we are kept informed as to how they are manipulated. Their defects, as well as their virtues, are discussed fully. The fashionable practice of loading data with a heavier theoretical superstructure than they are capable of bearing is scrupulously avoided. Instead of glittering generalizations of only passing interest, we have something of far more enduring value, a piece of fundamental knowledge about a very important aspect of U.S. history. It is regrettable that the work was not finished in time for inclusion in the 1960 edition of *Historical Statistics of the United States*. Those who use that volume are on notice that the early wage data are supplanted by Long's findings.

Walter Galenson

University of California, Berkeley

Wages in the Metropolis: Their Influence on the Location of Industries in the New York Region. By Martin Segal. Cambridge: Harvard University Press, 1960. Pp. xi, 211. $4.75.

The New York Metropolitan Region Study, a pioneer series that is surveying the economic and demographic elements in the country's biggest metropolitan area, has developed several types of economic analysis. In the initial volume, *Anatomy of a Metropolis,* it presented an over-all view of the region's manufacturing economy. It probed deeply into three of the area's key industries in the second, *Made in New York.* (The third monograph dealt chiefly with social problems.) Now, the fourth work in the series, *Wages in the Metropolis,* employs still another approach: an examination of the economy from a particular vantage point, namely, the role of wage levels and labor skills in influencing the location and development of industries within the region.

Segal drew a difficult assignment, for relatively little statistical data prepared by the government or private research organizations readily lend themselves to comparisions between wages in metropolitan regions. The result was a highly critical and selective use of printed sources of information, supplemented by dozens of interviews with business men, union leaders, and government officials. The latter undoubtedly gave him an invaluable apperceptive basis for evaluating the statistics on which his findings rest.

The industrial structure of the metropolis is never static—some industries leave the region or succumb altogether; at the same time, new industries gain a foothold. This study indicates that the region is most likely to lose manufactures or branches of manufacturing whose operations have become so mechanized or standardized that they can employ unskilled and low-wage operators. Industries which require rapid service, which involve rapid changes in fashion or design, or which turn out a highly variable product are most likely to remain. The region has also become a nursery for a wide variety of industries in the formative and experimental stage, for they require a large pool of skilled craftsmen as well as patent experts, legal advisors, and financiers. No comparable area in the country is as well equipped to fulfill these needs.

Wages in the Metropolis has deepened and broadened our understanding of the economic forces that tend to attract and disperse industries in the region, but it has not added a new dimension to our knowledge of the subject. Readers of the volumes that preceded it in the series will find little that is startlingly new. Let me hasten to add that this fact in no way reflects on the intelligence, expertness, or zeal on the part of the author, for these qualities are all conspicuously demonstrated in this work. Like all who venture into new and undeveloped areas, some researchers find a rich lode of materials; not all are equally fortunate. By studying the subject from a particular angle, Segal has confirmed previous findings in the series and added an increment of knowledge about the economy of the region, aptly called the "archetype of the American metropolitan community."

FREDERICK SHAW

The City College

Trade Unionism in Underdeveloped Countries. By SUBRATESH GHOSH. Calcutta: Bookland Private, Ltd., 1960. Pp. ii, 410. Rs 20.

Trade unionism in South East Asia, India, and Pakistan is the phenomenon under examination. The author's twin goals are to determine how trade unions are affected by economic development and "to what extent . . . organised labor can help in hastening the process of growth itself in a developing economy." The book is a description of trade unionism in the area and a blueprint of the kind of unionism that he would like to see created there. He carries the history of the labor situation from the beginnings of unionism through 1956. In writing the book the author has leaned heavily on his experience and contacts at the I.C.F.T.U. Asian Trade Union College in Calcutta.

In addition to a good social and psychological analysis, as well as a reasonably good economic analysis, of the labor-management situation in the region

studied, Ghosh presents his own theory of the origin of unionism. He finds it in the strains resulting from capital accumulation and the distribution of its burden.

Unions in Southern Asia have developed under the influence of the national liberation movements. Accordingly, the leaders are usually nationalistic politicians. The unions are generally small in size (*e.g.*, in India during 1953-54 the average size was 641), frequently one-shop affairs, possess infinitesimal treasuries, and rely very much on "outside" leadership. Rival unionism plays an important role. In Indonesia, India and Burma political parties control the unions. The shop-steward system is completely absent, and chronic unemployment exerts its influence. In a free test of strength with the employers, who have a Kohler-type mentality, the unions would generally come out second-best. As a result, the Asian unions depend heavily on government help.

Among the ways the author suggests by which unions can aid economic development are the following: First, by making themselves strong in order to defend and promote the workers' interests, they will create self-confidence among the workers "and will remove from their minds the sense of being exploited" (p. 391). Second, the unions may establish and aid workers' education programs, cooperate with the government or employers in getting workers to undertake vocational training, and effectively participate in joint productivity councils. Third, capital formation could be promoted by the unions through "promoting and assisting in the small savings schemes" and "by organizing voluntary labor squads for road building and other similar projects" (p. 392).

Establishing educational-and-recreational centers, organizing active recreational programs, and creating decent cooperative housing in conjunction with government or industry, are all ways in which the unions can aid the recently uprooted labor force to adjust to industrialization and urbanization.

The author emphasizes the fact that insufficient finances as a result of inadequate dues collection is one of the major weaknesses of Asian unions. Yet he too readily dismisses the check-off as a technique, his reason being that the employers would thus gain knowledge of the membership and financial strength of the union. This would only be significant as an argument in the early stages of unionization and certainly does not outweigh the advantages of the check-off to an established union.

From time to time Ghosh makes references to the American scene, in a significant number of which he demonstrates a shallow knowledge of the American labor movement. Thus he says that the development of collective bargaining activities in the United States dates back only to the National Labor Relations Act of the 'thirties (p. 287).

While discussing the influence of foreign organizations, foreigners within the Asian countries, and returning Asiatics, the author remarks, "It is interesting to note in this connection that the trade union movements in Europe and North America were comparatively far less influenced by foreign influence in the early stage of their development" (p. 66). In this he fails really to appreciate the influence of foreign-born leaders in establishing the American needle trades unions, maritime unions, and those of the building trades, to

mention but a few. In addition, the role of Samuel Gompers should never be underestimated. The various socialist internationals also played key roles in Europe.

Although there are some shortcomings, a number of which have been pointed out, Ghosh's book is a valuable contribution to the literature. It is a storehouse of information on Asian unions. His emphasis on the human elements involved in development, together with his good social and psychological analysis of labor-management relations in the area, make his book one that should be read by serious students of economic development and of comparative labor movements.

EDWARD ROSENBAUM

Tel Aviv, Israel

Employment Relations Research: A Summary and Appraisal. Edited by H. G. HENEMAN, JR., L. C. BROWN, M. K. CHANDLER, R. KAHN, H. S. PARNES, and G. P. SHULTZ. New York: Harper, for the Industrial Relations Research Association, 1960. Pp. ix, 226. $3.50.

This volume is the companion to the Industrial Relations Research Association's *A Decade of Industrial Relations Research, 1946-1956,* published in 1958. Like its predecessor, *Employment Relations Research* is intended to summarize research findings and note the gaps in our knowledge in basic areas. The title, unfortunately, is both inexact and forbidding. A work of this sort can be useful if carried out successfully. There are pitfalls: the essay may degenerate into either an uncritical catalogue of the literature or a generalized statement of the author's views with little regard for the literature.

Employment Relations Research is an uneven work which suffers from both of these defects. It contains two distinguished contributions—Herbert S. Parnes on the labor force and George P. Shultz and Arnold Weber on technological change; a successful summary by George W. England and Donald G. Paterson on selection and placement; a catalogue on compensation by David W. Belcher (361 footnotes!); and vaporous essays by David Dolnick on the history and theory of the labor movement, and by Gordon F. Bloom and Herbert R. Northrup on dispute settlement. The Dolnick piece, despite its title, hardly deals with history at all. The Bloom-Northrup essay fails to review the literature and sets forth opinions concerning the nature of industrial conflict and its accommodation with which this reviewer heartily disagrees.

The Parnes summary is a testimonial to the remarkable advance in knowledge concerning the labor force and labor markets in the past generation. Some of his findings are of interest. Our present labor force concepts, worked out in the 'thirties, are intended to guide public policy in dealing with mass unemployment; they are not as satisfactory for the purpose of finding scarce manpower under a condition of full employment. There appears to be long-run stability in the total labor-force participation rate. But this conceals significant changes among demographic subgroups—declining rates for very young and very old men, increasing rates for women, etc. There is an inverse relationship between labor-force participation by married women and the income of their husbands. At the moment we do not know the relationship between em-

ployment and the labor force. Similarly, no one has yet successfully defined a labor market. Labor mobility appears to be greater in the United States than in Europe and most job changes seem to be "complex," that is, involve simultaneous shifts in employer, occupation, and industry, but not geography. The conventional factors influencing mobility—age, sex, race, occupation—appear less important than personal determinants about which nothing is presently known. Finally, Parnes writes, "All of the empirical studies of labor markets make it abundantly clear that there are wide departures between the actual labor market behavior of both workers and employers and the assumptions on which the traditional theory of wage determination and labor allocation is based."

Shultz and Weber devote special attention to the burgeoning literature on automation, much of it in the realm of science fiction. The new technology brings with it cleaner and safer working conditions accompanied by tensions stemming from the social isolation of the worker and the integration of the work process. The research on the employment effect is too narrow thus far to assess the impact of technological change on employment in the whole economy. The impact of automation on skills is mixed; the expectation of general upgrading has not been realized. The new technology seems not to have destroyed incentive wage systems, as many anticipated. Likewise, automation appears to have had less effect upon trade-union and management structures than some thought. The authors conclude, "A moratorium on unverified projections about the effects of automation on industrial relations now seems in order."

These two essays and the third on selection and placement give helpful summaries of contemporary research findings and indicate the gaps that remain.

IRVING BERNSTEIN

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TITLES OF NEW BOOKS

General Economics; Methodology

BAADE, F. Der Wettlauf zum Jahre 2000—unsere Zukunft: ein Paradies oder die Selbstvernichtung der Menschheit. Oldenburg: Gerhard Stalling, 1960. Pp. 304. DM 19.80.

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FELLNER, W. Emergence and content of modern economic analysis. New York: McGraw-Hill, 1960. Pp. xiv, 459. $7.50.

FREEMAN, R. E. Postwar economic trends in the United States. New York: Harper, 1960. Pp. viii, 384. $6.

This book presents a survey of postwar developments in ten different aspects of the American economy. After a brief survey of some general problems of American society and of economics in America, it describes developments in postwar monetary policy, income distribution, fiscal policy, labor problems, regional distribution, corporate enterprise, corporate capital investment, and international trade in the United States. This book is not designed to give an integrated picture of the postwar structure of the American economy; it is a collection of essays, each presenting a workman-like job and testifying to the high professional caliber of the Department of Economics and Social Science at MIT. The book should be very useful supplementary reading for students of economics.

SMYTH, R. L. Economics. Reader's Guides 4th ser. no. 3. New York: Cambridge Univ. Press for National Book League, 1960. Pp. 30. 75¢.

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BOULDING, K. E. AND SPIVEY, W. A. Linear programming and the theory of the firm. New York: Macmillan, 1960. Pp. ix, 227. $6.

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NOTES

Arthur F. Burns, National Bureau of Economic Research, has been appointed chairman of the American Economic Association nominating committee. Suggestions for officers of the Association in 1962 should be sent to him as early as possible.

The Association announces the publication of Volume 1 of the *Index of Economic Journals* covering the period 1886-1924. Volumes 2 to 5, for the period 1925-59, will be published during the Spring of 1961. Members of the Association are invited to take advantage of a prepublication offer of $10.50 for the complete set. After April 15, 1961 the price will be $25.00. Orders may be placed with the publisher, Richard D. Irwin, Inc., Homewood, Illinois.

Announcements

The Social Security Administration has announced the initiation of a Cooperative Research and Demonstration Grant Program which will provide support for research of significance to social security programs and social welfare. At least during the first year priority will be given to projects related directly to the reduction of dependency and to improvement in programs under the Social Security Act. Grants will be made to public agencies and other nonprofit organizations, not to individuals. Application forms and instructions may be obtained from Dr. Ida C. Merriam, Director, Division of Program Research, Office of the Commissioner, Social Security Administration, 330 Independence Ave., S.W., Washington 25, D.C.

The *Immigration Research Digest* is being published under the auspices of the Committee on Research and Studies of the American Immigration and Citizenship Conference. It is a mimeographed summary of important new research contributions to the knowledge of migration—both in the United States and internationally. There will be two issues a year prepared under the editorship of Professor E. P. Hutchinson, Wharton School of Finance and Commerce, University of Pennsylvania. The editor will appreciate suggestions for materials to be included in the series from persons interested in developments in the field of migration.

The annual national conference and convention of the American Institute of Industrial Engineers will be held May 11-13, 1961 at the Sheraton Cadillac Hotel in Detroit. The entire program is designed to provide a variety of modern practical management tools to industry in general. Information about the convention may be obtained from James E. McCartney, 12th Annual A.I.I.E. Conference, Room 29, 4181 Oakman Blvd., Detroit 4, Michigan.

Deaths

Nicola Garrone, emeritus, University of Rome.

Russell C. Leffingwell, Morgan Guaranty Trust Co., October 2, 1960.

William H. Martin, Pennsylvania State University, November 10, 1960.

Lewis E. Severson, Beloit College, December 5, 1960.

Retirements

Esther E. Nelson, Hunter College.

V. R. Wertz, Ohio State University, October 1960.

Visiting Foreign Scholars

Just Faaland, Chr. Michelsens Institutt, Bergen, Norway: visiting professor of economics, University of Michigan, second semester 1960-61.

John B. Heath, University of Manchester, England: Northwestern University, January to June, 1961.

Jürg Niehans, Zurich, Switzerland: visiting professor of political economy, Johns Hopkins University, February 1961-February 1962.

Joan Robinson, Newnham College, Cambridge University: Northwestern, Chicago and Purdue Universities, March and April, 1961.

Hugh Rose, Exeter University, England: visiting associate professor, Northwestern University.

Orhan Tuna, University of Istanbul: visiting professor, New York State School of Industrial and Labor Relations, Cornell University, 1960-61.

Promotions

William H. Andrews, Jr.: professor of economics, Indiana University.

William J. Barber: associate professor of economics, Wesleyan University.

Joseph A. Batchelor: associate professor of economics, Indiana University.

C. C. Bowen: assistant professor, Ohio State University.

Paul C. Clayton: associate professor, Ohio State University.

Robert C. Connor: assistant professor of production management, Graduate School of Business, University of Chicago.

Robert Eisner: professor of economics, Northwestern University.

Louis Fier: assistant professor, Brooklyn College.

Clifton M. Grubbs: assistant professor of economics, University of Colorado.

James B. Hendry: associate professor of economics, Michigan State University.

Harry Malisoff: professor, Brooklyn College.

James L. McKenney: assistant professor of business administration, Graduate School of Business Administration, Harvard University.

Janet K. Messing: assistant professor of economics, Hunter College.

Max G. Mueller: assistant professor of economics, Michigan State University.

Lester G. Telser: associate professor of marketing, Graduate School of Business, University of Chicago.

F. W. Tuttle: professor of economics, University of Florida.

Arnold Weber: associate professor of industrial relations, Graduate School of Business, University of Chicago.

Fred M. Westfield: associate professor of economics, Northwestern University.

Fred Witney: professor of economics, Indiana University.

Administrative Appointments

George K. Chacko: manager, operations research department, Hughes Semiconductor Division, Newport Beach, California.

Frank T. de Vyver: assistant provost, Duke University.

Morris E. Garnsey: chairman, department of economics, University of Colorado.

Blaine E. Grimes: chairman, department of economics, Ohio Wesleyan University.

George B. Heliker: chairman, department of economics, Montana State University.

Walter E. Hoadley: vice-president and treasurer, Armstrong Cork Co., Lancaster, Pennsylvania.

Richard A. Musgrave: chairman, department of political economy, Johns Hopkins University.

Louis B. Perry, Pomona College: president, Whitman College.

Richard K. Stuart, University of Maine: chairman and professor, department of economics, Whitman College.

John G. Turnbull: acting chairman, department of economics, University of Minnesota.

Charles J. Walsh: chairman, department of economics, Fordham University.

Michael T. Wermel: dean, College of Business Administration and professor of economics, University of Hawaii.

Appointments

Michael Belshaw: assistant professor of economics, Hunter College.

Glenn Burress: assistant professor of economics, University of Cincinnati.

Stephen Campbell: department of commerce and economics, University of Vermont.

Lowell J. Chawner, formerly U. S. Operations Mission to Korea: visiting professor, College of Business Administration, University of Washington.

Carl F. Christ, University of Chicago: professor of political economy, Johns Hopkins University.

R. L. Darcy, Oregon State College: Kansas State University.

Henry B. Eyring: research associate in business administration, Graduate School of Business Administration, Harvard University.

Milton Gilbert, Organisation for European Economic Co-operation, Paris: economic adviser, The Bank for International Settlements, Basle.

Albert L. Gray, Jr.: professor of economics, Baldwin-Wallace College.

Roy B. Helfgott, Pennsylvania State University: Industrial Relations Counselors, Inc.

John B. Henderson: Andrew Wells Robertson Professor of Economics, Allegheny College.

Randall W. Hinshaw: professor of international economics, Claremont Graduate School.

Howard T. Hovde: professor of marketing, College of Business Administration, Drexel Institute of Technology.

Subbiah Kannappan, Reed College: department of economics, Ohio Wesleyan University.

Jeremiah Kaplan: professorial lecturer in behavioral sciences, Graduate School of Business, University of Chicago.

Abdul G. Khan: visiting lecturer in industrial development, Graduate School of Public and International Affairs, University of Pittsburgh.

John Klein, Oklahoma State University: associate professor of economics, Fordham University.

Paul F. Lazarsfeld: visiting lecturer on business administration, Graduate School of Business Administration, Harvard University.

James R. Lewis, Ohio State University: department of agricultural economics, Colorado State University.

Harald Malmgren: assistant professor of economics, Cornell University.

Paul E. Merz: associate professor, department of economics, Southwest Missouri State College.

K. E. Miller: assistant professor of agricultural economics and research associate, Bureau of Business and Economic Research, University of Missouri.

Dick Netzer, Federal Reserve Bank of Chicago: economic consultant, Regional Plan Association, Inc.

Egon Neuberger: economist, The RAND Corporation.

Peter Newman: lecturer in political economy, Johns Hopkins University, spring term.

Harold C. Passer: company economist, Eastman Kodak Company, Rochester, New York.

Don V. Plantz, University of Kansas: associate professor, department of economics, Arizona State University.

G. David Quiria: petroleum economics consultant, Department of Northern Affairs and National Resources, Ottawa, Canada.

Leonard Rapping: assistant economist, The RAND Corporation.

Jerome Rothenberg: visiting associate professor, Northwestern University.

Gregor Sebba: professor, Graduate Institute of the Liberal Arts, Emory University.

Warner Sichel, instructor in economics, Western Michigan University.

Ezra Solomon, University of Chicago: director, International Center for the Advancement of Management Education, School of Business, Stanford University.

Benjamin P. Spiro: consultant on development banks, Inter-American Development Bank.

William O. Thweatt, International Cooperation Administration: program specialist, Ford Foundation, first at the International Program in Taxation at Harvard University, then to Katmandu as research and planning economist to the Planning Ministry of the Government of Nepal.

Leaves for Special Appointments and Assignments

Irving Brecher, McGill University: joint director, Williams College Project Office, Karachi, Pakistan, for one year, August 1960-61.

Alfred L. Edwards, Michigan State University: advisor of economics, MSU Project, University of Nigeria, September 1960 to September 1962.

Max E. Fieser, Arizona State University: research associate, University of Oregon.

George A. Fuller, University of Utah: Fulbright Lecturer, University of Helsinki, 1960-61.

Rolf Hayn, University of Oklahoma: United Nations, New York.

Walter W. Heller, University of Minnesota: chairman, Council of Economic Advisers to the President.

George F. Henning, Ohio State University: International Cooperation Administration consultant, Turkey.

Herbert B. Howell, Iowa State University: International Cooperation Administration to consult with the Argentine government on beef production, fall 1960.

James A. Maxwell, Clark University: staff appointment to carry out a study of proposal for extending federal financial assistance to state-local governments, Brookings Institution, spring and summer, 1961.

Lester B. McAllister, Beloit College: National War College, Washington, 1961-62.

Frank B. Miller, New York State School of Industrial and Labor Relations: visiting professor, University of Istanbul, Turkey, 1960-61.

John C. Murdock, University of Missouri: research project with Community Studies, Inc., Kansas City, September 1960-61.

Andreas G. Papandreou, University of California, Berkeley: director, Center of Economic Research of the Academy of Athens and economic advisor to the Bank of Greece.

John H. Smith, American University, Washington: professorial lecturer in statistics, Graduate School of Business, University of Chicago.

Wolfgang F. Stolper, University of Michigan: economic adviser to the Ministry of Economic Development, Federation of Nigeria, until summer 1962.

C. R. Wharton, Jr., Council on Economic and Cultural Affairs, Inc., Singapore region: visiting fellow, department of agricultural economics, Cornell University, fall term 1960.

Arthur A. Wichmann, University of Wichita: assistant program officer, International Cooperation Administration, Burma, from September 1959 for two years.

Resignations

Sanford Bacon: Wharton School, University of Pennsylvania.

Dewitt C. Dearborn: Graduate School of Business, Harvard University.

Myra Janco: School of Business, Indiana University.

Richard Rainey: The RAND Corporation.

Williard E. Stone: Wharton School, University of Pennsylvania.

Miscellaneous

Peggy Bieley, formerly of Julian Langner Research, Inc.: has formed her own firm Peggy Bieley Research Associates, Inc., Miami, Florida.

Raymond Coleman: resigning as dean of the College of Commerce, West Virginia University, June 1961; after leave of absence 1961-62 for research under Food Foundation grant, will return as professor of economics and management.

Nestor Marquez-Diaz, Loyola University of the South: has opened a business and economic consulting office in New Orleans.

Richard S. Thorn: has been transferred from the European Office of the International Monetary Fund to the Western Hemisphere Department, Washington, D.C.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are Feburary 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Economist: A leading financial institution offers opportunity for an economist with graduate degree, doctorate preferred, and a strong background in money and banking, forecasting, and national income analysis. This position requires the ability to conduct independent research on a wide variety of problems bearing on Company operations. Starting salary $7,300-$8,500, depending on education and experience. Please send résumé giving full account of professional background. All replies will remain strictly confidential. P230

Economist: Opening June 1 or September 1, 1961, for a rapidly growing state college located in beautiful southern West Virginia. Doctor's degree required. Division of Business staff consists of eleven young faculty members and an enrollment of more than 300 majors. Concord College is accredited by the North Central Association of Colleges and Secondary Schools and the National Council for Accreditation of Teacher Education. Apply to Dr. Cloyd P. Armbrister, Chairman and Professor, Division of Business, Concord College, Athens, West Virginia.

Economics, principles and one advanced course: Ph.D. or all work completed except thesis (others will be considered) for eastern Catholic men's college, February or September, 1961. Instructor's rank, $4,000-$6,000; assistant professor, $5,000-$7,000. Rank and salary according to education and experience. P232

Accounting: Desire person with at least a master's degree, practical experience and teaching experience in accounting; C.P.A. certificate would be desirable. Rank from instructor to associate professor. Salary range $5,500-$7,500. P233

Economist: To do research and consultant work with the Department of the Treasury, San Juan, Puerto Rico. Position will be on Civil Service basis. Ph.D. degree, or advanced work leading to it, required; specialization in public finance and/or money and banking desirable. No previous experience required. Annual salary $7,200 plus personal travel expenses to and from Puerto Rico. Persons interested please write to the Director, Office of Economic and Financial Research, Department of the Treasury, P.O. Box 4515, San Juan, Puerto Rico.

Economist: Catholic college in Eastern Canada desires M.A. or Ph.D. to begin in September, 1961. Specialization in one or two of the following fields: money and banking, labor, statistics. Rank (lecturer or assistant professor) and salary according to education and experience. P234

Economist: To fill new position of Director of Institutional Research at all-male liberal arts college to carry forward projections made in recent preliminary study by the college of its long-range plans. The Director will execute research projects concerning the general operation of the college, collaborating with faculty and administration, and should have sufficient interest in the purposes and problems of the liberal arts college to be able to identify problem areas and initiate research. Salary range $7,000-$9,000, depending upon education and professional experience; substantial fringe benefits. Ph.D. in economics desirable but not absolutely essential. College teaching experience in a liberal arts context highly desirable. Address inquiries to Frederick deW. Bolman, Jr., President, Franklin and Marshall College, Lancaster, Pennsylvania.

Head, Department of Economics: New department of economics at rank of associate professor or professor. Ph.D. in economics, teaching experience, publications, and experience in directing doctoral dissertations are required. Salary approximately $10,000-$12,000 for eleven months. Midwest, September, 1961. P235

Economics and business administration: Small, fully accredited, church-related (Lutheran) liberal arts college. Candidates must have a Ph.D. in the field of economics and business administration; preparation should include economic theory, history of economic thought, accounting, finance, management, and marketing. Business and teaching experience are highly desirable. Salary and rank depend upon preparation and experience. Full-time faculty member for position beginning in September, 1961. P236

Accounting and business statistics: Central Pennsylvania liberal arts college with eight-man business and economics staff seeking man to teach various accounting courses and elementary business statistics. Accounting courses include managerial, cost, tax, auditing. Will also teach occasional management courses. Salary $5,000-$8,000, depending upon qualifications. Normal load is 12 hours. P237

Business administration: School of business administration in Washington, D.C. Openings in industrial management, international business, marketing, finance, personnel management, behavioral sciences. Ph.D. required. Ability to work with business groups, use case method preferred. Rank and salary dependent on qualifications. P238

Economists Available for Positions

Econometrics, mathematical economics, agricultural economics: Man, in late twenties; Ph.D., large midwestern university. One year of industrial research experience; publication. Desires teaching and/or research position in university. E907

Marketing research, international trade, finance, economic development: Man, 36, married; candidate for M.A. (Economics). Eight years of sales experience; over 3 years of experience marketing research and analysis consumer and agricultural goods; 1 year Census experience. Bilingual English-Spanish. Desires research position. Will relocate. E913

Principles, economic theory, econometrics, statistics, mathematics (through differential calculus): Man, 24, married; completing M.S. (economics) in January, 1961, at large midwestern university. Research and/or teaching position desired for 2-3 years before returning for Ph.D. James D. Clokey, 17-6 Ross Ade Drive. West Lafayette, Indiana.

Economic and political theory, economic principles, sociology: Man, Ph.D. Germany. Very seasoned teacher; many years of residency in America. E915

Economics, finance, management: Man, 50, married; Ph.D. Business experience; diversified teaching experience, including administration; various publications. Desires teaching, administrative, or research position. E916

Economic development, international economics, business economics, economic analysis, industrial planning: Man, 34, married; B.S., M.B.A., major New York university; requirements for Ph.D. completed pending dissertation. (Dissertation, on land and economic development, based on a case study of Italian economic development, completed this year under a Fulbright grant and an Italian government award.) Fluent in several languages. Seeks position in economic research and/or teaching college economics. Will accept foreign assignments. E917

Public finance, history of economic thought, economic principles or theory: Man, 48; Dr.rer.pol. from German university. Teaching experience in foreign country. Seeks position in college and/or in research. E921

Economics, statistics, marketing, marketing research, farm economics: Man, 37, married; B.Sc. (University of Copenhagen), M.A. (Iowa State), Ph.D. (Cornell). Ten years of experience in commercial research, teaching, and consulting. Will consider academic teaching combined with research, business, or government. Would go abroad. Available immediately. E924

Business statistics, mathematical statistics, statistical quality control, traditional and mathematical economic theory: Man. Six years of teaching experience. Available in June, 1961. E925

Economic theory, international economics, Soviet economics, national income analysis, transportation and industrial economics: Man, 33, married; Ph.D., Columbia University. Six years of progressive research experience in transportation, industrial input-output studies, and national income analysis. Currently employed with a national research organization. Available for teaching and research in the spring, 1961. E926

Contemporary economics and history of economic thought, money and banking, labor economics, business fluctuations, international economics and trade: Man, 30; M.A., currently writing Ph.D. dissertation. Presently teaching in a large state university but desires a change. Interested in teaching and/or research. Available in June or September, 1961. E929

Economics, finance: Man, 42. Eleven years of teaching experience on college-university level. Physical location at present, better salary considerations, and desire for more specialization in chosen fields are basic reasons for wanting to relocate. Available in September, 1961. E930

Economic principles, statistics, industrial organization, finance: Man, 37, married; completing all courses for Ph.D. during 1960-61 school year; holder of National Defense Fellowship. Seeks part-time research or teaching in southern California area for 1961-62 school year while writing dissertation. E932

Labor economics, labor law, business law, government and business, marketing, business management: Man, 32; M.B.A. (labor relations), LL.B. Three years in sales office management; 5 years of teaching. Seeks teaching position with or without administrative duties. Available in September, 1961. E935

Principles, monetary theory, international economics, national income analysis, comparative economic systems, price theory: Man, 38; B.A. (French), B.S. (Commerce), M.A. and Ph.D. (Economics). Nine years of teaching experience plus one year government experience; dissertation on International Monetary Fund. Teaching experience includes wide variety of courses. Desires teaching position in college or university or research position with public agency or private organization. E939

Principles, economic theory, economic policy, money and banking, national income, accounting: Man, 35, married; M.A., Ph.D. completed except thesis, which will be finished by next September; fellowship and research student at L.S.E. Teaching and business experience; presently visiting assistant professor in a midwest university. Would like a teaching position. Very good references available. E941

Industrial organization and public policy, industrial location, regional science, economic theory and history, monetary economics, economic development: Man, 39, married; Ph.D., Harvard University. Eleven years of teaching experience, combined with research; publications. Seeks college or university teaching or teaching-research position. Available in June or September, 1961. E942

Business administration, economics: Woman; Ph.D. Available for teaching and research in September, 1961. E943

Business administration: Man, married; B.A. in chemistry, M.S., Ph.D. in business administration. Diversified teaching experience in the areas of finance, management, and mathematics; business experience as consultant, business manager, and purchasing agent. Currently associate professor and department head. Desires teaching and/or administrative position. Available in June or September, 1961. E946

Economic principles, labor economics, labor law, public utilities, economic thought, comparative economic systems, U.S. economic history: Man, 30, married; B.A., M.A., Ph.D. dissertation in process. Five years of full-time college teaching; member of state panel of arbitrators; recipient of research grant; experience as research assistant, job analyst, supervisor of State Department project; instructor for evening adult school. Desires position at liberal arts college or university. Available in September, 1961. E947

Economic principles, Latin-American history, comparative systems, resources, international economics: Man, over 45; Ph.D. (1959), 100 graduate semester hours in economics. Five years of teaching experience; many years as federal official doing economic work with residence abroad; fluent in Spanish. Not an econometrician. Author of book and coauthor of another. Desires 12- or 15-hour-load teaching position with tenure in temperate U.S. area or West Indies. Saturday, night, or off-campus work at extra pay only. Salary open. E948

Economic principles and theory, business and economic statistics, quality control, managerial economics, business conditions analysis: Man, 36, married; B.S., M.A., Ph.D. dissertation nearly finished. Six years of successful university teaching. Seeks teaching position in nonmetropolitan area, preferably in the northeastern states. Available in September, 1961. E949

Economics: Man, 45; M.A., Ph.D. (economics). Years of teaching experience; Ford Foundation grant. Now on a university faculty. Desires teaching or research position in Southwest or West with above-average responsibilities. E952

Economic principles, income analysis, history of economic thought, price theory, labor economics: Man, 40; Ph.D. from large eastern university. Several publications in well-known journals. Now teaching at liberal arts college in Midwest. Desires academic position with opportunity for research. Available in September, 1961. E954

Marketing, labor, government finance, government and industry, national income—business cycles, history of economic thought: Man, 44, married; Ph.D. Seventeen years of college teaching experience. Desires teaching position in a college or university in the northeast. Available in September, 1961. E955

Economic development, international economics, money and banking: Man, 41; Ph.D., Columbia. Extensive experience in South Asia (India and Pakistan) and Western Europe. Publications in U.S. and abroad; recipient of major grants; knowledge of French, German, and Italian. Thirteen years of graduate and undergraduate teaching experience at major universities. Desires to make change. Minimum: associate professorship. E956

Economic principles and theory, financial analysis and planning, business economics, systems and procedures: Man, 27; completing master's dissertation in economics off-campus (all course requirements completed). Currently with leading research foundation. Desires half-time teaching and/or research position with university, or half-time to three-fourths time research position with business firm, trade association, or government agency. Planning to continue studies toward doctorate in economics on a part-time basis. Available in September, 1961. E958

Economic theory, monetary theory, cycles and forecasting, econometrics, statistics: Man, 31, married; Ph.D. Experience as corporation economist, consultant, and graduate and undergraduate faculty member. Book, monographs, and articles. Seeks teaching, research, or industry post with professional challenge and potential. West, Northwest, or Midwest preferred. Available in September, 1961. E959

Econometrics, statistics, mathematical economics, history of economic thought, government and business, institutional economics: Man, 27, married; Ph.D. just completed at large midwestern university. Will consider academic teaching combined with research opportunity, business, or government. Available in June or September, 1961. E960

Principles, labor economics, history of labor, comparative economic systems, personnel: Man, 35, married; Ph.D. to be received in June, 1961, from large midwestern university. Three years of successful college teaching; 2 years director of internship program in industrial relations. Seeks teaching position with opportunity for research. Available in September, 1961. E961

Money and banking, economic theory, statistics, business administration: Man, 41, married; B.S. in Mechanical Engineering, M.B.A., currently writing Ph.D. dissertation. Seeks position in economic research and/or teaching. Available in June or September, 1961. Prefers northeast. Arthur Robert Dorsch, P.O. Box 3507, University Station, Gainesville, Florida.

Investments, finance, international relations: Man, 57; M.A., M.S., Ph.D. Broad business experience as well as teaching experience, including administration as department head; various publications; presently professor of finance; excellent references. Desires position teaching above subjects. Available in fall, 1961. E962

Economics, monetary economics, labor economics, social security systems, statistics, macro and micro theory: Man, 38, married; Ph.D., Wisconsin. Eight years of teaching experience; 3 years as head of department; 2 short publications; some tax research. Would like to locate with reputable school which would allow him to teach one-half to two-thirds load so as to permit further study in mathematics and econometrics; excellent references. Available in September, 1961. E963

Management, labor, economics: Man, in 40's, married; Ph.D., economics-management, Big Ten. Experienced with accreditation, curricula, academic policies and procedures, graduate students. Continuous references of industrial and university experiences. Desires to locate permanently among good faculty environment. E964

Labor: Man, 43, married; M.A. in economics, U.C.L.A., 1959. Employed past year and a half with private firm doing research and arbitration case work for unions; 20 years of union experience as organizer, business agent, grievance committeeman, and member. Maintenance machinist with 10 years at trade. Seeks position that will utilize this combination of training and experience. E965

Labor and industrial relations, social security, economic theory and thought, comparative economic systems, international economics, public finance, European and American economic history: Woman, married; Ph.D. expected in 1961. Two years of experience teaching at a university on a temporary appointment; 3 years part-time; concurrent adult extension courses; publications. Desires teaching at smaller liberal arts college or combination teaching and research appointment in industrial relations. Free to move. E966

Economic theory, international economics, Soviet and Chinese economics, economic development and growth, industrial organization, public finance, monetary theory, business cycles, economics statistics, mathematical economics, marketing, market research, price policies: Man, 33; M.S. (marketing), Ph.D. (economics), postdoctoral study. Widely diversified and progressive experience in teaching and research, East, South, and West; publication; Ford Foundation grant. Desires teaching and/or research position requiring rigorous standards of professional training. E967

Economics, finance: Man, 40, married; Ph.D. in economics. Eleven years of successful teaching experience in both eastern and western universities; 3 years of government research; reads, writes, speaks French fluently. Desires challenging research-teaching position. Available in September, 1961. E968

Soviet economics, comparative economics, principles, history of thought: Man, 32, married; B.A., M.A., Ph.D. dissertation in process. Research experience in the field of Soviet economics. Some college teaching experience. Available in September, 1961. E969

Industrial and mineral economics: Man, 35, married; B.Sc. Honors, M.A. course requirements completed and thesis in progress. Extensive experience in applied economics, Canada, U.S., Europe. University research; publications; languages. Requires position in U.S. or abroad as company or research economist. E970

Money and banking, international trade: Man, 31; Ph.D. course work completed. Seventeen months of experience as director of research for a banking association; 1 year on an international trade project; good knowledge of Spanish. Desires a staff position in research or administration with private industry or a teaching-administrative position with a university. E971

Economic principles, labor economics, U.S. economic history, public utilities, money and banking, international economics: Man, 26; B.S. in education, M.A. in economics, several courses toward Ph.D. Four years of teaching experience. Desires college teaching position. Available in June or September, 1961. E972

International economics, economic development, foreign trade policy, money and banking: Man, married, retiring from international organization. Extensive teaching experience at leading universities in U.S. and Europe; published books on international and monetary problems; economic research covering many fields and firsthand experience in Asia, Europe, and Latin America. Desires full or partial teaching load in university with opportunity for research and publication. Available in fall, 1961. E973

Principles, statistics, international economics, transportation and public utilities, economic theory, money and banking: Man, late 20's; Ph.D. Seven years of college and university teaching experience, including present position and 2 years of governmental research; publications in print and in progress. Desires teaching position with good opportunity for advancement and with teaching load which permits research. Available in September, 1961. E974

Marketing, statistics, economic analysis, money and banking, international economics, public finance, history of economic thought: Man, married; Ph.D. credits completed. Nearly 15 years of responsible professional experience in directing and conducting economic and marketing research for management. Fellowship; university teaching. Seeks teaching or business position. E975

Economic development, business fluctuations, history of economic thought, economic principles, economic history: Man, British trained economist with Ph.D. Experienced teacher in English and American institutions; some publications; excellent references. Seeks post with teaching and research facilities, preferably in Pacific Coast or northeast region. Available in September, 1961. E976

Industrial and labor relations, economic principles, economic history: Man, 38; A.B., M.S., Ph.D. dissertation in process. Two years of university teaching; 1 year adult education. Salary and location open. E977

Principles, managerial economics, business cycles, history of economic thought, money and banking, economic theory, public finance, corporate finance, industrial management: Man, 35; Ph.D., 1959. Many years of successful college teaching; now department head, economics and business, of a small liberal arts college. Interested in teaching (or administration) in qualified college or university with excellent library facilities. Available in June, 1961. E978

International economics, economic development, macroeconomics, public and business administration: Man, 29; Ph.D., Columbia University, M.B.A., Harvard University. Three years of teaching experience at graduate and undergraduate levels; economist with an international organization; some industrial experience. Available in July, 1961. E979

Economic theory, labor, finance: Man, in 40's. Twenty years of postdoctoral research, writing, and teaching. Primarily interested in graduate level instruction and research. In *Who's Who in America, Who's Who in Commerce and Industry,* etc. E980

International finance and economics, general business administration, international relations: Man, American, 26; M.A. requirements to be completed in June, 1961. Two and a half years of graduate study, in top 1 per cent of undergraduate graduating class. Twenty-seven months previous residence and travel in Africa (Ethiopia) and Middle East; currently residing in Europe; recent personal visits to the ECSC, the Council of Europe, NATO, and the EEC; good knowledge of technical French. Desires a position with an organization with international interests. James E. Ammerman, Box AMM-J, Bologna Center of Johns Hopkins University, Largo Alfredo Trombetti 3, Bologna, Italy.

Industrial and labor relations, personnel administration, labor problems, collective bargaining, training: Man, 37, married; B.A., M.A., working on Ph.D. Eight years of industrial and labor experience; 1 semester of university instruction. Desires new position with private industry or university. Will relocate. Salary $12,000. E981

International economics, foreign marketing, finance, business cycles, international economic development: Man, 32; M.A., Harvard University, M.B.A., Wharton School. Industrial and financial management experience; record of successful administration and co-ordinated planning. Wishes opportunity to participate in foreign economic development and international monetary field. E982

Industrial management labor relations, personnel management, labor economics, production courses, industrial engineering: Man, married; Ph.D. candidate, dissertation scheduled for completion in March, 1961. Extensive experience includes 14 years of teaching, industrial consulting, labor arbitration, research supervision, and administration plus industrial employment. Desires teaching, research supervision, or administrative position in university. E983

International economics, economic development: Man, 64, married; Ph.D., George Washington University. Thirty-five years of experience in U.S. government, balance-of-payments and foreign investment research, international economic problems and relations. Foreign experience. Desires teaching position, preferably small university. Available in fall, 1961. E984

Economics, money, banking, and finance, accounting, business law, management: Man, 56; J.S.D., Ph.D. Experienced college economics teacher; professional experience in law and accounting practice. Norman S. Lehrman, 1300A Midland Avenue, Yonkers, N.Y.

Principles, economic theory and history, public finance, resources: Man, late 40's, married; Ph.D., 1960, M.A., Columbia. Fifteen years of writing, research, and administration in utility and oil industries and government. Desires teaching. Available in June or September, 1961. E985

Labor economics, statistics, cycles and forecasting, theory: Man, 38, married; Ph.D. Ten years of college teaching experience; presently chairman of department. Seeks teaching, administrative, or research position in large eastern city. Available in June or September, 1961, or September, 1962. E986

International economics, economic geography, Soviet and satellites economy: Man, 47; M.A., currently writing Ph.D. dissertation. Studies in Europe and here; 7 years of research experience; 8 years of business experience in foreign trade; German, French, and Slavic languages. Available for teaching and research in spring, 1961. E987

Business management, marketing, public finance, investments, economic principles, business history and trends: Man, 45, married; M.A., work toward Ph.D. Fifteen years in business management; assistant to chief executives. Also some teaching, newspaper and magazine editorial work, and city planning; presently management consultant; publications; conservative leanings. Wishes to devote full time to teaching and writing. Available after June, 1961. E988

Economic principles, labor economics, money and banking, introductory business statistics: Man, 36; M.B.A. (industrial relations), Ph.D. (economics). Four years of teaching experience. Seeks college teaching position in Southwest, West Coast, or South. Available in September, 1961, E989

Intermediate Income and Growth Theory

by MELVIN L. GREENHUT, University of Richmond, and FRANK H. JACKSON, University of Hawaii

Explains what national income is, why it behaves as it does, why an understanding of its behavior is important and what can be done about it. The objectives are achieved by tracing the historical development of the theory, by introducing its essence in highly simple form and by systematically introducing the concepts necessary to understand and use the theory as a dynamic tool of analysis.

1961 *376 pp.* *Text price: $6.25*

The Measurement of Social Welfare

by JEROME ROTHENBERG, Northwestern University

What should be the criterion by which social welfare is measured? In answering this question, the author analyzes present difficulties and examines in detail psychology and other social sciences from an economics viewpoint. The purpose of the examination and analysis is to find ways that welfare economics can be made useful.

August 1961 *Price to be announced*

Money, Debt and Economic Activity, 3rd ed.

by ALBERT GAILORD HART and PETER B. KENEN, both of Columbia University

A systematic account of American monetary arrangements, the theory of money and the relationship of money to economic instability. The text covers banking organization and operations, non-bank credit institutions and public debt, business fluctuations, international monetary relations and economic stabilization problems.

May 1961 *512 pp.* *Text price: $7.95*

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CONTENTS

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